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A World History

By WILLIAM H. McNEILL, Chairman, History Department,
University of Chicago

One of America's most distinguished scholars chronicles world history in a concise survey, presenting a cohesive and intelligible view of the history of mankind. As in his earlier volume, the award-winning *The Rise of the West*, Professor McNeill places stress not on what happened, but on why it happened and on the ideas that have shaped civilization.

The organization of the book follows the shifting world balance among cultures as, first in one place and then another, men succeeded in creating an unusually attractive or powerful civilization. Each epoch of world history is surveyed by studying first the center or centers of primary disturbance, then examining the reactions of other peoples to what they knew of the innovations which had occurred in the prime centers of cultural creativity. Thus, as the broad relationships of world civilizations are delineated, the unity and continuity of mankind's past is clarified and the differing life styles of each of the major human societies are carefully distinguished.

In the perspective of the text, geographical settings and lines of communication between different civilizations become centrally important. Professor McNeill draws from archaeology, technical history, and art history since these sources often provide important clues to ancient relationships which sometimes far exceed what the surviving literary record can indicate.

Richly illustrated, the volume contains forty-eight pages of halftones and a twenty-page section of world maps in color. Chronological tables, forty line maps within the text, and a bibliographical essay at the end of each section further augment the text material.

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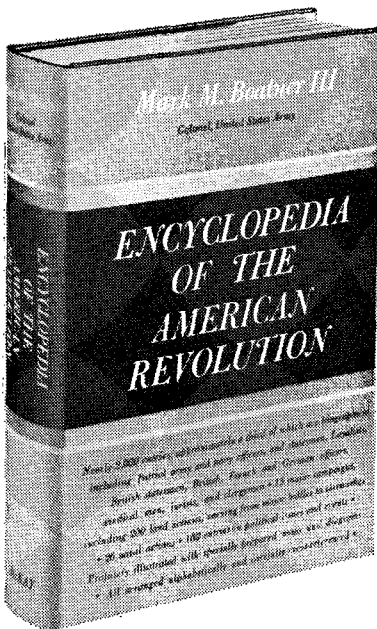
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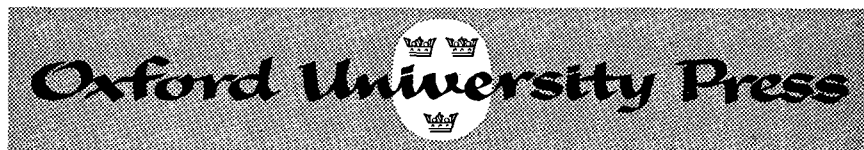
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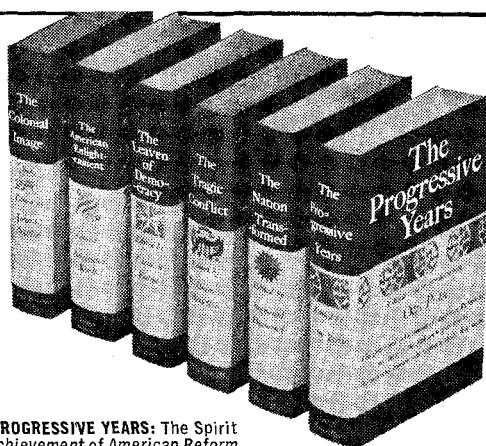
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JANUARY 1967

History in a Self-Governing Culture

ROY F. NICHOLS*

FORTY-SIX years ago tonight the American Historical Association held a session such as this in the city of Washington. Upon that occasion the late Edward Channing delivered the address. As a young man he had been present at the first meeting of the Association in 1884, and, presumably, he had heard the speech delivered by Andrew D. White, the first President. As a graduate student attending my first meeting of this organization I listened to Channing in 1920. (This represents a cycle of sorts.)

When President White spoke, the world, in sharp contrast to the present, seems to have been relatively quiet. England was engrossed in the Victorian Age, secure in the midst of great possessions. The unification of Germany and Italy had been accomplished. The French Republic had achieved stability. Russia had recovered somewhat from the recent shock of the assassination of an autocratic tsar, and along the Danube an *Ausgleich* had produced a conglomerate Dual Monarchy. At home Reconstruction had been officially accomplished, and the Grant scandals were a thing of the past. To

* Mr. Nichols, professor of history emeritus at the University of Pennsylvania, delivered this presidential address at the Hilton Hotel at Rockefeller Center, New York City, December 29, 1966.

some the only cloud on the horizon was the possibility of the election of Grover Cleveland and the return of the Democrats to power. White reflected to some extent this comparative calm when he urged his associates to contrive a philosophical synthesis of human affairs in a large, truth-loving, justice-loving spirit. He reminded his hearers in terms familiar to-day that, unless historians engaged in the study of general history and the history of civilization, their specialized research would be barren and often worthless.

When Channing spoke to his post-World War I audience, much had happened since the days of President White. The United States had become a recognized world power, the home of hitherto almost undreamed of wealth and progress. The nation had gone to war to make the world safe for democracy, and wreaths of victory bedecked the Allied standards. Channing himself was just completing his fifth volume dealing with the history of the United States from 1815 to 1848 and was deep in thought about volumes and years to come. He was particularly conscious that his native Massachusetts was beginning the commemoration of the third century of its experience, for the *Mayflower* had arrived just three hundred years before. Never a man of contagious optimism, Channing seemed somewhat dubious that night. He outlined the great progress that the nation had made in the last hundred years, but concluded with the question:

In all this, in the evolution of the greatest industrial society that the world has ever seen, have we gained or have we lost? Are men and women to-day happier and better off, politically, spiritually, mentally, morally, and physically, than our ancestors were in the days of James Monroe, John Quincy Adams, John C. Calhoun, Henry Clay, Daniel Webster, and Andrew Jackson?¹

I cannot recall that there was much evidence that the American Historical Association shared in these doubts and regrets; certainly Channing did not arouse any observable overt response save the hope that he might live to finish his great work. Tonight we meet not in the nation's capital, but in its great metropolis, as we again confront the task of recording, analyzing, and interpreting an age, this time nearly half a century removed from that just alluded to.

As these words are written in 1966, the task of recording, analyzing, and interpreting this different age has become a much more demanding one. When Channing spoke in 1920 we were approaching the end, had we but known it, of the Progressive Age. This age had dedicated itself to the dogma

¹ Edward Channing, "An Historical Retrospect," *American Historical Review*, XXVI (Jan. 1921), 202.

that man by taking thought could perform miracles. Disease and misgovernment seemed to be on the way to being vanquished. A great war had been brought to a triumphant conclusion, a war not only to make the world safe for democracy but to end war itself. A great League of Nations had been created to maintain all these vaunted ends. The lion and the lamb were to lie down together, and peace was to reign for a thousand years. But even in December 1920 it was evident that the lion and the lamb were not too compatible. Whether the somewhat florid Warren Harding could carry the banner of the broken Woodrow Wilson could be, and was, doubted. Perhaps Channing himself reflected something of the malaise that was increasingly apparent. He had only finished his fifth volume, and for him, too, time was running out.

At the time of Channing's address the historical interest that had long prevailed in the profession was in the process of change. The long-time preoccupation with political and constitutional history was challenged by scholars holding a concept of social history represented by the editors and authors of the "History of American Life Series," and this was being reflected in a growing number of courses and dissertations in the graduate schools. Those at work in political history were aware of a novel interest displayed by some of their fellows plowing new furrows. Several varieties of specialization in fields such as economic, social, and intellectual history grew increasingly attractive, and the process of fragmentation moved on apace. The capacity to generalize suffered, and the aphorism that graduate students were learning more and more about less and less was oft repeated. Certainly political history had lost some of its general interest.

This fragmentation demonstrated a weakness and a need which, unless they were met, were bound to impair the capacity of American historians to synthesize and thus to interpret. As a political historian I was conscious both of the decline of activity in this field and of the growing lack of comprehension to which the fragmentation was contributing. Could not some unifying tendency be developed, some counterinfluence in the way of interpretation and generalization be discovered and encouraged?

But fragmentation was not the only, nor was it perhaps the major, influence on a changing historiography. It was at this time that an interest in biography was attracting historians. Popularly this often included a taste for debunking, but professionally it caused scholars to become more interested in the psychiatric approach and in a more analytical study of the human beings whom they examined and portrayed. A greater realism marked the writing of the day.

A third influence grew apace. The so-called behavioral disciplines were

burgeoning among the social sciences, and this Association was included in the Social Science Research Council in the early days of its development. Social anthropology, social psychology, and sociology were developing interests and concepts that the historians slowly began to appreciate. This appreciation, undoubtedly hindered by semantic complications caused by the efforts of these scholars to invent a new terminology, led to what some historians might consider outlandish neologisms. Interest was developed in group dynamics, in the behavior of small groups, in mass psychology, in competition and cooperation, and in other forms of human behavior with which the historian must on occasion be concerned.

The use of scientific analogies likewise continued. Certain patterns of thought developed by the natural scientists had attracted or repelled social scientists and humanists since the days of Isaac Newton and John Locke. Within the memories of many of us, historians had been looking for law, for dynamic interpretation. Edward P. Cheyney developed a concept of law in history. Henry Adams professed to see in the physicists' second law of thermodynamics the doom of man's intellect. Others pondered over concepts of relativity, uncertainty, and the immaterial nature of matter. Historians in some instances were no longer so confident that they could discover just how *eigentlich* things might have *gewesen*.

During this period of fragmentation, however, the tradition of synthesis was by no means forgotten. There was a deliberate interest in continuing it and, incidentally, in arresting the chaotic influence of specialization and fragmentation upon the historian's capacity to generalize. Various efforts were made in the early years of the twentieth century to restore the desire and the power to scan wider horizons. One of these was the use of a civilization concept such as employed in different ways by Arnold Toynbee, Charles and Mary Beard, and later by William McNeill. Another very significant instrument for this purpose was the culture concept borrowed not from scholars in belles-lettres, but adopted in broader terms from social anthropologists. As I have found this cultural concept one of the most useful aids available to historians in developing the synthesis, which is one of their main responsibilities, I propose to dwell upon some of its implications for members of this Association.

The term "culture" used in this sense is all-inclusive, embracing as it does all the behavior patterns employed by any given society. It also supplies the concept of a unity greater than even the sum of its definable parts. Into such a synthesis can be fitted any specialized, any personal, or any national experience. Each of us can relate his interest to any such concept of image,

national character, or *Gestalt* that appeals to the individual's sense of the all-embracing. Viewing any specific problems in the light of such over-all interpretation supplies whatever each of us may do with a maximum of significance and interpretive meaning.

There are various types of cultural definitions, but one in particular can be especially useful: namely, I believe, the design most indicative of the nature and the identity of any society. This is its plan of operation, the force or influence that organizes it and keeps within it a semblance of recognizable structure and order. In highly complicated societies this plan takes the form of government, the customs of rule, of the exercise of authority, of the structure of power. A culture therefore may be known as a democracy, an empire, a totalitarian state. Any such designation is not merely derived from constitutional institutions, but it embraces attitudes, ideas of community identification, and social as well as political relationships. The distinguishing characteristic of the society known as the United States of America is the fact that it is a democratic culture dedicated to a self-government in which all are technically involved and in which this interest is demonstrably central to the self-identification of the people. It can be used as the hallmark of the culture.

This use is appropriate likewise because a basic, if not the basic, historical problem in this culture is how a society expanding so quickly in so large an area became and has remained self-governing. The study of this problem has involved me and many others in working out the process of the evolution and the operation of this culture and has also concentrated much interest on a particular phase of it, namely the stresses and strains that eventually led to the destructive social war of 1861-1865, which nearly destroyed it.

This culture concept, which has dominated and determined the history of the United States, is broader and older than the boundaries of the United States would imply. It has involved consideration of the European origin of folkways and institutions. The proper definition of this broader field of analysis is the Anglo-American culture, including much that evolved in the British Isles and was transported to America. There it was transplanted and eventually matured in a new society. The employment of this very broad culture concept is extremely useful not only in overcoming the disadvantages of fragmentation but in quickening the capacity for synthesis. Likewise, it has sharpened our understanding of historical process by giving greater opportunity for more sophisticated conceptualization of certain of our historical responsibilities.

Setting significant chronological limits to the study of the evolution of the Anglo-American cultural patterns shows how old many of these patterns

actually are. Customs of local self-government and representative lawgiving and lawmaking processes involving the beginning of election procedures go back into the "good customs of the realm" of England, some of which originated among the British tribes and antedate history. Folkways deriving from the various invasions of Britain and from phases of the religious transformation of the sixteenth and seventeenth centuries stimulated popular interest in participation in government and produced the beginnings of something like political parties. In other words, the basic patterns of community organization and self-consciousness, together with an operating power structure of self-government, were created and developed in England.

During the period of the folk migration across the Atlantic Ocean that resulted in the establishment of the American colonies, these cultural patterns were transported across the sea. Consequently, much of the time there was little invention involved: the migrants took what they knew, imported it when they could, and used it with a minimum of adjustment. This process of adaptation therefore emphasized the sense of age that characterized the customs of even those in a new world.

The American phase of the Anglo-American cultural evolution was separated by both time and space from the parent culture in a fashion difficult to understand in this age of television and jet propulsion. This distance and the difficulty of communication meant that the same qualities of intrepidity and enterprise that brought colonists across the sea would stimulate their long-accustomed habits of self-government to the point where they would employ another cultural pattern long in use. Not only had the British developed a habit of community self-rule and a search for consensus, but there was also a pattern of violent change in government. Regicide and revolution in some form had occurred in England about once a century from the days of the Norman Conquest, and in the seventeenth century two full-fledged revolutions, the Puritan and the Glorious Revolution, had occurred, the latter with an interesting American phase that indicated a growing jealousy of American self-governing autonomy and promised that perhaps a light weight would be assigned to European authority.

The possibly inevitable rupture came at the end of the eighteenth century when, in the interesting years of the Age of Reason, American colonial leaders, impatient at certain limits imposed on their cherished autonomy, found themselves involved in violence just about a century after the Glorious Revolution of 1688. Their experience with the responsibilities and confusions of complete autonomy subsequent to 1783 led the erstwhile colonies to seek a new surrogate in place of the crown and Empire. They constructed a curiously wrought instrument that they invented probably at

the only time and place where such a feat, up to this point, could have been contrived. They created a federal system in which the power structure was divided and modified by a series of checks and balances described in an unprecedented document, the Constitution of the United States. Despite their care, however, its authors did not foresee the problems involved in the choice and conduct of those exercising power, particularly executive power.

The new federal republic was to have its capacity for order and definition of identity complicated by its ecology. Its area was so great, its population so scattered, and its wealth and variety so fabulous that the question began to concern the thoughtful few: could such a people so situated practice the art of self-government? Was the surrogate that was substituted for the crown adequate?

The size of the republic and the variety of the ecological characteristics soon demonstrated that the so-called political federalism described in the Constitution was much more intricate than a mere political federalism. The American society was a cultural federalism, not so much a federation of political units fused into a republic as a federation of varying groups identified by attitudes, customs, and community associations and combined in a society. This variety of elements produced differences of views that in turn could be used as points of dispute and debate in the periodic elections required to determine the exercise of power decreed by the constitutions, federal and state.

As the decades of the nineteenth century wore on, it became apparent that this product of the Age of Reason was not too successful in dealing with all the confusions of this rapidly growing state, and the question began to arise as to whether the confusion was not reaching a point at which it would no longer be possible for even the capacity of this self-governing society to control it and to maintain orderly self-government. Had conditions tending to disorder reached a degree at which it was no longer possible for men to control them? In 1861-1865 the outbreak of the social war seemed to indicate that such was the case—just about one century after the last appreciable outbreak of violence, the American Revolution.

At the moment we happen to know much about this last episode because we have just passed through a four-year period of centennial commemoration in which many of the historical guild actively participated. And from this four-year period we have learned much about the nature of this Anglo-American culture pattern, particularly its now predominantly American phase. We have learned, or we should have learned, a great deal about the antecedents of violence and the phenomena that accompanied its out-

break. Perhaps in 1966 it may be well to make reference to some of these attendant circumstances.

In the first place it is appropriate to point out what may not appear to be a truism to all of us: that we are too prone to think backward in history and to shape conclusions by what we find in the past. To use the example of 1861-1865, when the bitter war between the Union and the Confederacy was fought, it was easy to assume that this conflict between two well-defined forces must have been inevitable and have been inexorably building up during a long range of time variously defined. But the closest kind of study of the outbreak of the conflict and the antecedent years presents evidence that the dominant situation was confusion. There was not one South but several, and in the end the South presented by no means a united front in the Confederacy. And it can hardly be said that there was any North until Sumter. In fact, so great was the confusion that there were not just two alternatives, war or peace, but several. It is convenient to characterize this situation as the operation of a third force, for wherever there seems to be a convergence of two forces there are probably one or more others at work that may at any time intervene and produce a different combination. In the past this third force had on occasion been some form of compromise, and so usual had been this intervention that many in 1860 felt sure it would operate again. The confusion was too great, however, and until the last minute a variety of alternatives might have taken over. In the end I feel that a series of accidents rather than destiny, or great forces, or any *deus ex machina* decided the issue. Eventually, after bitter and expensive conflict, the republic righted itself—but that was a century ago.

This description of the concept of American self-governing culture, which we have used in our search to overcome fragmentation and to re-establish synthesis, and of the observable tendency to periodic resorts to violence to change our patterns of self-government, not only indicates an interesting paradox, but also gives us food for thought as we ponder certain problems which today's necessities call upon us to face and which we may illustrate by asking ourselves certain questions in this year of 1966.

Since that holiday season when Channing asked his questions, nearly a half century ago, many more things have changed than the fashions of history writing about which we have just been thinking. Many new conditions of life have appeared and have added much to the complexity of our task and thus to our responsibility. Now wars seem never to end, and where there is no war there may be racial tension, for a world-wide reorientation of peoples is in process. The energy of the atom has been released; population

is exploding. Time and space seem no longer to be limiting concepts; man not only stands on the verge of outer space, but on occasions walks in its vastness. A mechanism is replacing the human brain in some forms of computation. We are told that the genetic code has been broken and that the universe has lost its parity and is lopsided. We are reminded that upon occasion when some basic irregularity, operating contrary to the accepted laws of the universe, has been discovered, such observation can be the prelude to significant new knowledge. A drift in the perihelion of the orbit of the planet Mercury was eventually accounted for when the general theory of relativity was formulated. With all these aberrations and incongruities may we not be on the eve of certain discoveries in human knowledge? At any rate this wide variety of new circumstances carries us far away from the days of White and Channing, and certainly into a period of new responsibility.

In the light of this possible new experience we are justified in asking questions, even as Channing asked them—though they will be of a different nature. The main question that we raise is this: if we are on the verge or in the midst of certain basic changes, not only within our culture but within the cultural structure of the world, what should be the centers of our scholarly interests? Further, if we grant that there are few questions more significant than whether self-government can be maintained in the world in general and in the United States in particular, we have a closely correlated series of queries.

Among today's circumstances most vigorously suggesting questions of this character are a number connected with the power situation in our self-governing culture. The desire for power has intensified in the lives of so many people. Rivalries, conflicts, tensions are everywhere and seemingly increasing. Despite our pride in our capacity for self-government, we may at times wonder whether we are keeping up. There are signs that we have a confidence in our political capacity that may have been more adequate in other times, and that we may be depending upon a changing power and status.

For our security's sake do we know enough about the history of this American self-governing culture of ours, of the nature and the location of the power that is operated within it? Many calls are made upon us for the use of our power from within and without our society. And there is no certainty as to our answers to these calls. Much in fact is at stake. For if we exert too little power, we may become anarchic; if we contrive too much, we could become totalitarian. In either case we would be abandoning our basic culture pattern.

Do we know enough about the evolution of our power of self-government? Can political institutions defined in the Age of Reason remain adequate in an age when reason is more in the background? Do we know enough about and do we understand the process of power choice and power change as revealed in our history? Do we understand our changing techniques of choice and decision making, of our attitudes toward the responsibilities of self-government, of the dynamics of our political emotions? Do we have adequate information about the types of people who seek political power and how they achieve it? Do we realize sufficiently the dangers involved in operating self-government in times of increasing change? Do we understand the changes and the rhythms in the exercise of the powers of self-government? Are we aware of the implications of the rhythm of our power distribution, caused by our custom of inducing an artificial crisis every four years by putting the executive and legislative power up for possible change?

Do we realize that every so often confusion can increase to the point where it threatens men's understanding of and capacity to handle it, as in 1861-1865? What within the historian's range of recognition of events and behavior trends can he identify and interpret for society's benefit, because of his capacity to think in time? Our experience in 1861-1865 was that in those times there were so many signs pointing in so many directions that the observers and the analysts of that day were quite beyond their depth. Do we not have a greater capacity for observation and analysis today, and are we using it?

In times such as these when there are signs of shifts in the power structure that could prove drastic, is it not essential to know the nature of the relationship between prevailing custom and the power structure? It does little good to any society to think that it is behaving as though it were independent and self-governing and then to wake up to find it is not.

Finally and most important, as political change is generally determined by cultural change, have we command of sufficient skill in directing human mechanisms to make certain that we calculate and operate an efficient adjustment of political to social change so that self-government can proceed with safety?

When Channing asked his questions in 1920 they were phrased in terms of achieving and maintaining human welfare and happiness. The questions we ask today are rather expressed in terms concerned about continuing efficient self-government and ensuring its survival.

Having asked these questions, how well equipped are we to answer

them? To do so, we need more than a skill at simple narrative. These complex times demand sophisticated analyses that will place new obligations upon us to develop our intellectual potential. Do we have it at our command?

We have a much more formidable foundation of fact at our disposal upon which to erect a structure of interpretation and synthesis. So many more historical facts are now available. We know about many more individuals and types of individuals. Much more information has been collected by statistical techniques. The behavioral sciences have mobilized much knowledge about human behavior. Advances in the natural sciences are again tempting the historian into the alluring realms of *Geisteswissenschaft* as he dreams of discovering genetic codes shaping the destiny of societies and wonders about the effects of living in a universe that has lost its parity, in an age sometimes called absurd.

Not only have we many more facts at our command, real or fancied, but there is new equipment in the realm of gadgets. We are entering the years of the computer. Now million-dollar machines are housed in computer centers and tended by programmers. It is too soon to be very dogmatic about how useful this type of automation may be, but historians are so far behind in their reporting and analysis that we must investigate to the best of our ability its potential as an instrument of historical retrieval and conceptual discovery. We are so frustrated for want of a break-through that no device should go untried.

Meanwhile, we must not forget that we can experiment with certain fresh concepts, new patterns of thought. Individual scholars should be concerned with much greater spans of time. Evolution is an extremely slow process. Its slow-moving changes involve much more of adaptation than innovation. There are much less orderly process and much more haphazard confusion in human behavior. There is a greater variety of alternative action possible in even the simplest program set by determining forces. In fact, there are much less determinism and a higher frequency of accident in the processes of change.

Historians, fortunately, not only have these data, these instruments, and these concepts, but a growing number of scholars are using them. Within the last few years there has been a veritable renaissance in the study of this principal element in the American self-governing culture, namely the power structure and its fluctuation. Numerous studies encouraged by the Social Science Research Council and its committees, supported by foundation grants, are being carried out at the Harvard Center for the Study of the History of Liberty in America, at the University of Michigan Consortium,

at California, at Illinois, at Rutgers, and at the University of Pennsylvania, and elsewhere. These projects are much more comprehensive in type than the more narrowly defined political and constitutional history studies current at the beginning of the century. Much attention is paid to the manifold influences and subtle character of the determinants of this basic behavior. Research of this type is contributing much to our ability to answer the questions we are raising in 1966.

While we enjoy advantages such as these to aid us in answering the questions we are now raising, we should, nevertheless, be aware that we are not doing all that we should to prepare the next generation to use such advantage in the quest. We may advisedly give more thought to the educational policies and programs of our graduate schools, particularly at a time when so many new such schools seem to be springing into being. More than a century ago we took over the Ph.D. program from the German university system almost without thought and with little adaptation, and we have too long avoided its reconsideration. Our current Ph.D. training often falls between two stools. It might be a degree in course, such as a law school degree, or it might be an adventure in freedom, wherein, after an experience of discovery, the student presents his results for evaluation and judgment. The program that we usually follow, however, can well be a *tertium quid*. Herein a certain number of requirements are set, exercises prescribed, and examinations administered. Undue emphasis is placed on the learning process—learning under observation, and surveillance if you will. There is too little opportunity to fashion concepts of the history of human behavior in the form of problems that must be formulated, analyzed, and understood. In these days of increasing enrollments in graduate schools, numbers seem to enforce a formalized program that can be administered almost by tabulation without very much individual attention. To the gifted this often means a degree that provides less than it should; to a considerable number it may mean a degree more prestigious than they deserve. Graduate faculties may well ask themselves if there should not be two degrees so that in one of them there may be real opportunity to develop more elements of sophisticated analysis, such as are suggested by these questions, while the other should concentrate upon methodology and practice both in teaching and research.

Also I think graduate faculties may well expand their thinking into the realms of postdoctoral facilities in universities. There should be more opportunities available to scholars who have been teaching for a while to come back to the university for a few weeks or for a term or two to study intensively new techniques and new findings and to engage in colloquia on

current problems in historiography. Here there would be new acquaintances and new ideas, new dialogues designed to disseminate the fruits of wider experience and increased maturity in a university environment.

The use of these various instruments just considered in our efforts to answer the questions we have raised should give us a sense of our increased intellectual capacity developed during the life of this Association. We are peers in the realm of the mind. We have a discipline and a series of unique functions of our own. These instruments of analysis, these forms of thought are our own, and we owe them to none but ourselves. Other scholars have neither devised them nor used them with any common degree of frequency. An interesting commentary is the observation that historians themselves do not use them as they might. They fail often to recognize their own philosophic strength or to achieve their intellectual potential. They have a tendency to depend upon their colleagues in other disciplines for instruments and patterns of thought. Yet they have their own intellectual birthright, an autonomy they should never forget.

Our too common dependence upon others we may ascribe to our folklore about history. To so many history is just a story of the good and the bad, of the great and the small. In school it is something to be learned at the expense of loss of interest in the story. In graduate years it becomes an exercise in the higher criticism of sources, a carefully organized narrative based upon tested facts. And for so many that is all that it ever is, and life may be spent in producing a series of doctoral dissertations, each more careful and perhaps more specialized in a narrow field than the last. To a certain extent this is highly commendable, for the historian who can put a good narrative in literary form based upon a comprehensive survey of the sources and an accurate recording of facts has indeed done much.

But we can and should do more, and it is unfortunate that we do not. Simple, specialized narratives of limited national experience, though they produce much of interest, do not give the historian free play for the wide use of his mind or the development of his intellectual potential. The historian often stops too soon. His life can be, and I believe should be, one of growing capacity to discover, understand, and communicate satisfying analyses of human behavior, and not, as too often happens, a temptation to abdicate his most meaningful function. The historian should furthermore use his wisdom and his imagination to advance hypotheses, to project himself beyond his tested data and conclusions based thereon, and to establish advanced positions in the world of research, even at the risk of having to admit on occasion that he may be wrong. As in so much of prime signifi-

cance for human existence, ingenuity, daring, and cultivated strength are major essentials.

As we seek the answers to these questions of 1966, we as historians are dependent on no one for our philosophical instruments; as we think in time we devise our own, and by their use we can experience the past, reconstructed and relived in our own consciousness. We owe it to ourselves, therefore, to use these instruments of our own invention in an original fashion, confident that thereby we can discover truth perhaps obscure to others. If we do this to the extent of which we are capable, we may provide the knowledge indispensable for the successful continuance of our culture.

A half century ago Channing asked his troubled questions in terms of the possibility of progress; today we ask them in terms of the possibility of survival. If we are to answer these questions with any degree of success, we must recognize that we have an intellectual capacity of our own, not fully realized, which we can develop. To do so we must declare our philosophical independence and raise the standard of our own intellectual identity.

Jacopo Aconcio as an Engineer

LYNN WHITE, JR.*

THE early sixteenth century in Europe witnessed two revolutions, both of which altered habits of the previous thousand years and each of which, by the latter 1500's, had crystallized into patterns that remained nearly intact until the end of the nineteenth century. One was the Protestant Reformation and the defensive response to it in the regions still loyal to Rome. The other was a sudden and profound change in military technology, the chief element of which was the development of light, highly mobile cannon that shot iron balls in fairly flat trajectories. Since the older style of fortification crumbled before such devices, an entirely new, and enormously costly, apparatus of defense was required.

It would be hard to decide which of these simultaneous revolutions had the greater impact on European life, or the more lasting effects. Because of the dominant value structures in our culture, there is an immense literature on the Reformation and Counter Reformation, whereas the history of warfare from the standpoint of the professional soldier or the military engineer—types traditionally alien to the humanistic scholar—is still largely ignored. Yet it is doubtful whether the “Cartesian” mentality, which assumed that mathematics is the key to reality, would have become dominant if Europe had not been assiduously bankrupting itself by building new military defenses in which assurance of safety was achieved less by tangible masses of masonry than by abstract geometrical patterns of lines of fire.

Jacopo Aconcio, because he was both a significant reforming theologian and a reputable engineer, is particularly worth pondering as one of the few men¹ who participated creatively in both of these upheavals subverting two aspects of European life and thought. While much is known of his religious activity, the present state of scholarship has permitted us only casual glimpses of his technological work and connections.

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¹He was not unique even among Italian Protestants. Rocco Guerini, count of Linara, a Tuscan of intense religious interests, studied engineering at the court of Ferrara, took service with Francis I of France in 1545, and became Inspector General of Fortresses. In 1558 he was the chief French engineer at the siege of Thionville, and from 1561 to 1564 he planned and built the new citadel of Metz. He had become a Protestant, however, and eventually fled to Germany where he was appointed the chief engineer of the Elector of Brandenburg. (See Carlo Promis, *Biografie di ingegneri militari italiani dal secolo xiv alla metà xviii* [Turin, 1874], 520–26; R. Mazauric, “Un ingénieur huguenot au xvi^e siècle et ses rapports avec l’Église messine: Roch Guerini, comte de Lynar,” *Bulletin de la Société de l’Histoire du Protestantisme Français*, CVI [Oct. 1960], 173–85.)

Born in or near Trent at a date not even approximately known, Aconcio studied law, became a notary at Trent in 1548, lived for a time at the court in Vienna, and in 1556 became secretary to Cristoforo Cardinal Madruzzo, Charles V's governor of Milan. We have had almost no detailed information about his life or thought before 1557 when his religious views compelled him to seek safety among the Protestants of Switzerland, then of Alsace, and eventually of England. As a refugee he became well known, less for what he did than for what he wrote.

Aconcio is chiefly remembered today for having provided the first careful intellectual formulation of the religious basis for religious toleration. Skeptics and indifferentists might nibble at the edges of the millennial Christian axiom that force may rightly be used to extirpate heresy, but in a Europe still deeply committed to faith—or rather to faiths—they could have little effect. Sebastian Castellio's *De haereticis* (Basel, 1554), written in a fit of religious nausea at the atrocity of the burning of Michael Servetus, spoke primarily to the Christian heart rather than to the Christian mind. It was Aconcio's *Satanae stratagemata libri octo* (Basel, 1565)—a title natural to a military engineer—that first provided clear exposition of a Christian rationale of tolerance. W. K. Jordan is correct in saying that "Castellion is the Luther of the literature of toleration; Acontius the Calvin."² The *Stratagemata* continued to be widely read throughout the seventeenth century, particularly in the regions where religious toleration first began to win acceptance: the Netherlands and Britain.³

The essence of the new mode of fortification was the application of mathematics to its problems, and Aconcio's mental process was permeated by the mathematical method of starting from clear and concrete principles, and passing step by step to greater generality and simplicity.⁴ In 1558, at Basel, Aconcio published a treatise *De methodo, hoc est, de recta investigandarum tradendarumque scientiarum ratione* establishing his manner of reasoning. It was reprinted several times, and while there is no evidence that René Descartes knew it, this work was part of the European atmosphere that he breathed. In 1641 the Dutch Cartesian Huelner, writing to Marin Mersenne, having praised Descartes's analytical method, remarked that he had found nothing earlier to match it, except the little book *De methodo* by Aconcio.⁵

² W. K. Jordan, *The Development of Religious Toleration in England from the Beginning of the English Reformation to the Death of Queen Elizabeth* (Cambridge, Mass., 1932), 315.

³ The most recent, and moderate, evaluation of "L'influenza dei *Satanae stratagemata*" is in C. D. O'Malley, *Jacopo Aconcio* (Rome, 1955), 199–215; see also Jean Jacquot, "Acontius and the Progress of Tolerance in England," *Bibliothèque d'Humanisme et Renaissance*, XVI (No. 2, 1954), 192–206.

⁴ See Jordan, *Religious Toleration*, 318.

⁵ Adrien Baillet, *La vie de M. Des-Cartes* (2 vols., Paris, 1691), II, 138.

At the end of *De methodo*, Aconcio promised a series of works applying his method to various disciplines. The only systematic book of the sort that is known to us was produced probably between 1562 and 1564:⁶ a pamphlet on how to read history that has its own place of honor in the history of the philosophy of history. It was never published, but it appeared in slightly abbreviated English translation in his friend Thomas Blundeville's *The true order and Methode of wryting and reading Hystories, according to the precepts of Francisco Patricio, and Acontio Tridentino* (London, 1574), "the first separately printed treatise in English on the art of history."⁷

As C. D. O'Malley's admirable monograph shows, Aconcio either knew personally or corresponded with many of the most conspicuous figures of his time, ranging from the Emperor Maximilian II to Sir William Cecil, the indispensable man of Queen Elizabeth's regime. In Switzerland and Strasbourg he became acquainted with most of his fellow *fuorusciti* from the frustrated Italian Reformation as well as with the Swiss reformers and the Marian exiles. In England he enjoyed the patronage of the Earl of Leicester and the Earl of Bedford. He did theological battle with Edmund Grindal, then bishop of London. There he lived in the house of the Dutch historian Emanuel van Meteren and was the intimate friend of the Queen's influential Italian tutor, Baptista Castiglione, whom he appointed executor of his estate at his death in 1566 or 1567.⁸ To O'Malley's collection of evidences of his connections one item should be added: James Phillips of the English department at the University of California, Los Angeles, now engaged in editing the Latin poems of Daniel Rogers, son of the first Marian martyr and an international literary personage in his own right, finds that he dedicated a poem to Aconcio.

But what of Aconcio's professional life as an engineer? It is symptomatic of our ignorance of the history of technology that only one student of Aconcio has noted that he was brought to England in the autumn of 1559 by Elizabeth's new government explicitly because of his skill in fortification.⁹ It was necessary to import the revolution in military technology in order to defend the precariously re-established Protestant revolution.

Early in 1559, after the fall of Calais to the French, Antoine Perrenot,

⁶ O'Malley, *Aconcio*, 171.

⁷ Hugh G. Dick, "Thomas Blundeville's *The true order and Method of wryting and reading Hystories* (1574)," *Huntington Library Quarterly*, III (Jan. 1940), 149; see Jean Jacquot, "Les idées de Francesco Patrizzi sur l'histoire et le rôle d'Acontius dans leur diffusion en Angleterre," *Revue de littérature comparée*, XXVI (July-Sept. 1952), 333-54. An English translation was published by C. D. O'Malley as *Of the Things That Have to Be Observed and Taken into Account in the Reading of Histories* (Palo Alto, Calif., 1942).

⁸ O'Malley, *Aconcio*, 51.

⁹ Hugh G. Dick, "Giacomo Concio: A Renaissance Exile," *Modern Language Forum*, XXVI (Mar. 1941), 13.

Cardinal de Granvelle, rebuked the English ambassadors at Cateau-Cambrésis for England's neglect of the new military concepts—a neglect that had lost them their last foothold on the mainland. “The art of war,” he said, “is such that men be fain to learn anew at every two years’ end.”¹⁰ Indeed, the 1550's were a decade in which a cluster of novel ideas was consolidating into a new science of fortification.

While the increased availability of gunpowder for explosive mines compelled modifications of walls and moats to prevent sapping, the chief problems of static defense came from the amplified power of gunfire. By the 1480's the French had largely abandoned the huge, slow-firing cannon of the first 150 years of chemical artillery in favor of lighter guns that could be loaded more quickly, moved easily on a perfected gun carriage, and that fired iron balls. As Charles VIII demonstrated during his Italian invasion of 1494, even the most majestic medieval walls melted before the blaze of such assault.

After twenty years or more of confusion the Italians began to see that since any conceivable wall could be reduced to rubble, defense of a fortress must depend no longer upon its bulk or height but rather upon ingenious mathematical design which would prevent the attacker from exploiting the inevitable breach. Rounded towers, moreover, created “dead spots” under the walls convenient for attackers and difficult to cover by lateral fire. Likewise, square or acute angles of masonry were vulnerable to enemy guns. The answer to these difficulties, and the central invention of the new Italian style of fortification, was the bastion: a great blunt arrowhead-shaped bulwark projecting from each angle of the fortress. On the bastions were mounted cannon to sweep the whole length of the curtain walls between the bastions as well as to bombard the besieging force. Each bastion was connected to the main fortress by a short neck, or flanker, with lateral openings or casemates (normally protected from direct enemy fire by masonry ears, or orillions, on the bastion) from which artillery could rake the assault against a breached curtain. If the defenders had food, water, gunpowder, and morale, the lines of fire made possible by the new mode of military architecture seemed to make them impregnable.

The invention of the bastion has been much debated, as has been the origin of sloping (or battering) the walls, and of lowering them into deeper ditches to reduce the profile vulnerable to gunnery, or of systematically backing them with earth, or even building them entirely of earth and timber, to cushion the impact of cannonade. But whatever the bastion's

¹⁰ J. U. Nef, *War and Human Progress* (Cambridge, Mass., 1950), 30.

source,¹¹ its generalized application was Italian: it appears full blown in the new defenses of Verona built between about 1530 and 1548.¹² Yet even the Italians were hesitant to put so revolutionary a notion—that for security pure mathematical relationships are more important than lofty masonry—into print. The first book to announce the innovation is Giovanni Battista Zanchi's *Del modo di fortificar le città*, published at Venice in 1554. Within two years this was translated into French, although without credit to Zanchi, by François de la Treille, *La maniere de fortifier villes, chasteaux et faire autres lieux fortz* (Lyons, 1556).¹³

To their peril, the English were slow to pick up the new tricks, and some were as aware of this fact as was Cardinal de Granvelle. One of them was Robert Corneweyle, an Englishman interested in military technology who was in Paris in March and July 1559¹⁴ and who was sent back to England by Sir Nicolas Throckmorton, the English ambassador to France, on September 4, 1559, by way of Dieppe to spy out the fortifications and report to Elizabeth's Council.¹⁵ Corneweyle brought back to England more than scattered items of information. British Museum, Additional Manuscript 28,030 contains the first, and unpublished, description in English of the Italian mode of fortification: *The keye of the Treasorie: The Manner of fortificacion of cities, townes, castles and other places . . . translated out of the Frenche into the Englishe tonge but also corrected and augmented by Robert Corneweyle . . . addressed to lorde Ambrose Dudley, maistre and capteine generall of the ordinance of England 1559*. The preface is signed (fol. 4^r) by Corneweyle on December 31, 1559.¹⁶

¹¹ B. H. St. J. O'Neil, *Castles and Cannon* (Oxford, Eng., 1960), 67, suggests Cilicia; J. R. Hale in *The New Cambridge Modern History* (6 vols. to date, Cambridge, Eng., 1957–), I, 282, proposes Rhodes in 1496, but in his admirable "The Early Development of the Bastion: An Italian Chronology, c. 1450–c. 1534," in *Europe in the Late Middle Ages*, ed. J. R. Hale et al. (Evanston, Ill., 1965), 466–94, he concludes that it was indigenous to central Italy.

¹² Piero Pieri, *Il Rinascimento e la crisi militare italiana* (Turin, 1952), offers a large bibliography down to about 1530. Horst de la Croix, "Military Architecture and the Radial City Plan in Sixteenth-Century Italy," *Art Bulletin*, XLII (Dec. 1960), 263–90, explores the nature and implications of the military revolution of that era.

¹³ See Promis, *Biografie di ingegneri*, 401–403.

¹⁴ *Calendar of State Papers, Foreign Series, Elizabeth, 1558–1559* (London, 1863), 180, 383, 403.

¹⁵ *Ibid.*, 541. On August 2, 1560, Corneweyle wrote to Sir William Cecil that he was supervising the demolition of the fort at Dunbar; also that he had found there three French artisans who could make the new *arquebusses de calibre* which had from two to three times the firepower of the old. (*Calendar of State Papers, Foreign Series, Elizabeth, 1560–1561* [London, 1865], 208–209.)

¹⁶ The manuscript is a copy made after April 12, 1560, when Dudley was appointed Master of Ordnance, but before December 26, 1561, when he became Earl of Warwick. (See "Ambrose Dudley," in *Dictionary of National Biography . . . From the Earliest Times to 1900*, ed. Sir Leslie Stephen and Sir Sidney Lee [66 vols., London, 1885–1901], VI, 98.) I have not seen De la Treille's plagiarism of Zanchi, but believe that De la Treille was Corneweyle's French source, partly for lack of another candidate. Certainly this last was based on an Italian work: all examples of fortifications (except Tyre and Babylon!) are Italian (see esp. fol. 12^v). Yet Corneweyle makes the matter his own: on fol. 39^r he writes with great concern of the

In this work the intellectual and moral attitudes of a military engineer of Aconcio's time emerge: a sense of the challenge of peril and the exhilaration of progress in the art of war (fol. 2^r).

The malicious nature of man coulde never invent the thing so dangerous or hurtful . . . but that the nature, ingen, and spirit of the same bothe invented and found remedie against and for it . . . (fol. 2^v). The invention of the artilarie hathe doen us this good, to constraine us of force to doe our industrie to fynde the meane so to fortifie oure townes against it, that we might fynde suertie in them . . . (fol. 9^r). For why the auncients of tyme past builded their strengthes in other sorte and forme than nowe are builte? Because their ingens and instrumentes of warre weare other than theise in oure dayes. For now use we no more to overthrowe strengthes with Arietes nor Tortures; but with ingens of more force, violence and terror: as the Canon, the Culvering, and shott of sondery kindes. And in stede of their argines and valtes under the grounde, in theise oure dayes we approche with oure trenches, with oure moignes overthrowe.

The British Museum manuscript is equipped with admirably drawn diagrams, first in plan and then in perspective, of a variety of fortifications illustrating all the elaborations of the Italian style (including even ravelins), as well as examples of defective fortresses to demonstrate weaknesses.

Fortunately for England, Sir William Cecil himself shared Corneweyle's concern for the modernization of military architecture in Elizabeth's realm, to the point where he was personally reading the available treatises and searching for more. On October 12, 1559, he wrote to Throckmorton: "I pray you sir, let one of yours bring me such indices as be to be had for books in Paris that I may make my choice at your coming. I am now and then occupied in Vitruvius de Architectura; and therefore if there be any writers besides Vitruvius, Leo Baptista [Alberti] and Albert Durer (all which three I have) I would gladly have them."¹⁷

Cecil, however, had concluded that books were not enough; his country needed some good Italian engineers who had the skills in their heads. As early as 1545 sporadic examples of the Italian manner of fortifications had

capture of Boulogne and Calais by the French. As for the relation of Corneweyle's treatise to the first printed English book dealing with the new art of fortification, Peter Whitehorne's *Certain waies for the orderyng of Souldiers in battelray . . . and also fygyures of certain new plattes for fortificacions of Townes* . . . (London, 1562), it appears that the two Englishmen worked independently on the basis of the same French source. Whitehorne's diagrams are identical with those of Corneweyle, but are fewer. At the beginning of his technical discussion of the design of fortresses (fol. 15^v) Corneweyle says: "The proporsion that draweth nerest to the rounde is with reason preferred." Whitehorne starts his equivalent section: "The forme whiche unto the circular fation doeth most resemble, of experte and skilful soldiers, is above all other with moste reason praised." Unfortunately the best recent survey of Elizabethan military literature, Henry J. Webb, *Elizabethan Military Science, the Books and the Practice* (Madison, Wis., 1965), omits fortification and thus does not help to clarify such relationships.

¹⁷ *Calendar of State Papers, Foreign Series, Elizabeth, 1559-1560* (London, 1865), 34.

been built or planned in England, generally, it would seem, by foreigners;¹⁸ but the new method had not taken adequate root. By July 17, 1559,¹⁹ Cecil was already negotiating secretly, through Throckmorton, to secure for Elizabeth the services of Giovanni Portinari, a Florentine engineer who had worked for Henry VIII and Edward VI, had married an Englishwoman, had fallen under the royal displeasure, and had taken service with the French to build fortresses in Piedmont.²⁰ With France and England on the verge of hostilities, Portinari's effort to change allegiances was difficult; he did not arrive in England until sometime between May and October 1560.²¹

Anticipating possible disappointment in recruiting Portinari, Cecil had thrown his net wider. On August 25, 1559,²² Throckmorton wrote to Cecil saying that Aconcio (of whom Cecil's father-in-law, Sir Anthony Cooke, and Sir Thomas Wroth could inform him) had been at Paris for the previous eight days. He had a high regard for England and was well affectioned in religion. Aconcio would leave shortly for England; Cecil could get further information from the ambassador's letter to the Queen. (Unfortunately this one does not survive.) He added a postscript to Cecil:

¹⁸ Girolamo da Treviso, chiefly remembered as a painter, was employed by Henry VIII in 1538 as an expert on fortifications. He was killed, however, in 1544 during an attack on Boulogne, and we have no evidence of either plans or construction coming from him. (See A. E. Popham, "Hans Holbein's Italian Contemporaries in England," *Burlington Magazine*, LXXXIV [Jan. 1944], 13-14.) A fully developed arrowheaded bastion was built at Yarmouth in 1546-1547 to protect the two landward walls of the castle, but we do not know the architect. (See S. E. Rigold, *Yarmouth Castle* [London, 1958], 2, 6-7.) Probably it was designed either by Antonio da Bergamo or by Gian Tomaso Scala of Venice, both of whom were sent on January 27, 1545, to assist Sir Richard Lee on the fortification of Tynemouth. (*Letters and Papers, Foreign and Domestic, of the Reign of Henry VIII*, ed. J. S. Brewer et al. [21 vols. in 33 pts., London, 1862-1910], XX (1), 45.) Scala was still at work there on October 11, 1545, and Bergamo in July 1546. (*Ibid.*, XX (2), 266, 516; XXI (1), 648, (2), 447.) British Museum, Cottonian MS, Augustus I, ii, 7, preserves a plan for the defenses of Tynemouth bastioned in the Italian manner; it is covered with notations in Italian. H. H. E. Craster, *A History of Northumberland* (15 vols., London, 1893-1940), VIII, 157, long ago recognized that it can scarcely be Lee's work, but must rather be that of his Italian colleagues. The map is published by H. A. Adamson, "Tynemouth Castle after the Dissolution of the Monastery," *Archaeologia Aeliana*, 2d Ser., XIX (1898), 68, plate II; see *ibid.*, XVIII (1896), 62. A plan of the same fortification (strangely dated 1542) is found in Scala's unpublished treatise on fortification at Turin. (Biblioteca Reale, Manoscritto militare 377, fols. 13^v-14^r.) An autobiographical note (fol. 47^v) tells of his works in England: "fisi la forteza de timor [Tynemouth] che son sulla bocha de la fiumare de novo castel [Newcastle] et in quel ano fisi la traversa a barvich [Berwick] et fisi quel de uarch [Wark]." That Scala and Bergamo worked together on all these projects is indicated by a manuscript map of Wark Castle on Tweed containing notes in Italian and the name of Bergamo. (See *Calendar of the Manuscripts . . . at Hatfield House* [18 vols., London, 1883-1940], XIV, 305.) Before 1547 true bastions were built at Portsmouth. (See O'Neil, *Castles*, 68, plate 18b.) Since in the Turin manuscript (fol. 48^r) Scala says that he returned to Venice when Henry VIII died, he was not the engineer who, under Edward VI, planned the bastioned citadel at Berwick upon which construction was started before 1552. (See Iain MacIvor, "The Elizabethan Fortification at Berwick-upon-Tweed," *Antiquaries Journal*, XLV [No. 1, 1965], 65, fig. 1.)

¹⁹ *Calendar of State Papers, Foreign, Elizabeth, 1559-1560*, 383, 403.

²⁰ *Calendar of State Papers, Foreign Series, Mary, 1553-1558* (London, 1861), 78.

²¹ *Calendar of State Papers, Foreign, Elizabeth, 1560-1561*, 80, 351-52.

²² *Calendar of State Papers, Foreign, Elizabeth, 1558-1559*, 500.

Aconcio was not to be compared to Portinari for deep judgment, but of service in matters of fortification.

Aconcio, then, reached England during the early part of September 1559 to find himself in the curious position of being Cecil's stopgap, a second line of defense in case Portinari found the Channel too wide. But doubtless Aconcio was weary of wandering. As has been said, in 1557 his crypto-Protestant views made it necessary for him to flee from Milan to Switzerland.²³ Later, in the Strasbourg group of Marian exiles, he had met Wroth and Cooke. On December 20, 1558, within hours of the arrival of the news of Elizabeth's accession, these two departed for England,²⁴ clearly expecting major preferment in the new government. But Elizabeth did not immediately trust men who had once shown the conspiratorial mentality; those who had plotted against Mary could not penetrate quickly the inner circles of power under the new Queen. Cooke and Wroth waited impatiently. Aconcio, on arrival, would soon have discovered that he had less backing than he had expected. To strengthen his position he must seize the initiative. His gambit had considerable influence upon English patent law.

In December 1559 Aconcio petitioned the Queen²⁵ for a patent on the manufacture of "new designs of machines of all sorts that use [water] wheels, and a new design for building furnaces for dyers and those who make beer, and for other uses, with a great saving of fuel." The request is made in terms of public interest: those who, at trouble and expense, produce inventions that serve the commonweal should receive the privilege of monopoly as compensation.

The idea of rewards for invention had been known in Italy since 1421 when Filippo Brunelleschi was given a patent by Florence for a barge equipped with derricks.²⁶ In the rest of Europe, however, patents continued to be inducements for the introduction of alien skills (not necessarily new), stimuli to expand a given industry, or simply favors granted to the influential. Aconcio's petition of 1559 is the first statement in England of the modern concept of the purpose of a patent.²⁷

²³ For the context, see Federico Chabod, *Per la storia religiosa dello Stato di Milano durante il dominio di Carlo V* (2d ed., Rome, 1962).

²⁴ C. H. Garrett, *The Marian Exiles, 1553-1559* (Cambridge, Eng., 1938), 126, 346.

²⁵ *Calendar of State Papers, Domestic Series, Elizabeth, 1601-1603, with Addenda, 1547-1565* (London, 1870), 495; text in Paolo Rossi, *Giacomo Aconcio* (Milan, 1952), 18, n. 34; and O'Malley, *Aconcio*, 26-27: "novae rationes machinarum omnis generis quae rotarum usum habent, et nova ratio fornaces fabricandi pro tinctoribus et ijs qui cervisiam parant, et ad usus alios cum magno lignorum compendio."

²⁶ M. Frumkin, "Early History of Patents for Invention," *Transactions of the Newcomen Society*, XXVI (1947-49), 48.

²⁷ This is denied by D. S. Davis, "Acontius, Champion of Toleration, and the Patent System," *Economic History Review*, VII (No. 1, 1936), 63-66, but he does not produce a

Where did Aconcio get the idea for his petition of patent? Possibly from his general Italian background, but more immediately from his recent technical experience. As we shall see, from about 1549 until 1556 he had spent much time at the court of Charles V. In *De subtilitate*, published at Nuremberg in 1550,²⁸ Girolamo Cardano describes in detail a large cranked machine for sifting flour that had been invented about 1547. The inventor received a patent on it from Charles V, and since not only all the bakers but also clerical establishments and noble houses feeding many people secured such machines from him, he made a good living and "brevi tempore sibi domum edificavit"! Aconcio would certainly have known this episode, and to him, a lonely engineer in a strange land, it must have suggested an effort to solve his problems.

Perhaps because he had insufficient influence at court, or because he was a foreigner, or because his proposals were technically puzzling, Aconcio's patent was not awarded until September 7, 1565.²⁹ This grant is slightly more explicit about the nature of Aconcio's inventions than is the petition. The furnaces for making beer and dyes with a great saving of fuel—a major problem in Elizabeth's day³⁰—remain vague, but machines are mentioned for grinding, for crushing, for cutting wood, and for many other uses to which water-powered machinery is applied. Since vertical-wheeled water mills for a great variety of industrial purposes had long been routine in Europe, one suspects that Aconcio's inventions involved the horizontal water wheels which by the middle of the sixteenth century were the fashion with Italian engineers.³¹ In some cases the power was to be supplied by wind in such a way that, no matter what the direction of the wind, the mill would operate while the rest of the structure remained unmoved. Since the tower windmill, as distinct from the post-mill, had been known in the North Sea region since the later fourteenth century,³² Aconcio could scarcely have se-

clear case earlier than 1559 of either a petition for, or a grant of, a patent to the inventor of a device; nor does he successfully challenge the date of Aconcio's petition. The only earlier relevant petition may be that of George Brooke, a member of the great Kentish family of Cobham, together with a foreigner, Tomazo Chamata, for a patent on a new foreign dredge to clear sandbars from rivers or harbors. (The date may be about 1558, although the endorsement says 1550.) There is, however, no claim to its invention, and when at last the patent was granted, on May 26, 1562, Chamata had vanished and Brooke is said to have discovered the dredge "by diligent travel." (See E. W. Hulme, "The History of the Patent System under the Prerogative and at Common Law," *Law Quarterly Review*, XII [Apr. 1896], 145; Davis, "Acontius," 66.)

²⁸ I cite the edition of Basel, 1554, 72-73.

²⁹ *Calendar of Patent Rolls, Elizabeth I, 1563-1566* (London, 1960), 331.

³⁰ See J. U. Nef, *The Rise of the British Coal Industry* (London, 1932), 156-64.

³¹ See Ladislao Reti, "A Postscript to the Filarete Discussion: On Horizontal Waterwheels and Smelter Bellows in the Writings of Leonardo da Vinci and Juanello Turriano," *Technology and Culture*, VI (Summer 1965), 428-41.

³² The earliest picture of a tower windmill is in Bibliothèque Nationale, MS français 2810, fol. 260^r, of the end of the fourteenth century, published in *Livre des merveilles* (2 vols., Paris, 1907), I, plate 232; see p. 2 for date.

cured a patent on it. His proposal presumably involved some form of the vertical-axle windmill which appears sporadically in Italian engineering from the fifteenth century onward.³³

Finally, Aconcio was granted a patent on mills moved by water that need not be flowing; they are alleged to work where there is only a little well or a bit of collected water. Aconcio evidently thus patented, and perhaps first conceived, what was to become one of the favorite perpetual motion devices of the seventeenth century: it was first published by Vittorio Zonca of Padua in 1607.³⁴ A siphon tube of varying diameter, with its lower and smaller end (the intake) in a pool of water, has a large bulge in its shorter, downcurved end (the mouth) which is above a water wheel. Since nature abhors a vacuum, the greater gravitational thrust of the water in the bulge, as it empties down onto the wheel, draws the lighter column of water upward from the pool to replenish itself constantly. Thus the wheel turns merrily because the water flows everlastingly in a vertical loop.

It may be assumed that no one in England tried to build this remarkable machine, because Aconcio's reputation and status rose rapidly. On February 27, 1560, he was granted a royal pension annually of sixty pounds, retroactive to the prior Christmas.³⁵ On October 8, 1561, he became an English subject.³⁶ But money continued to be a problem for Aconcio, and his engineering skills offered solutions. Early in 1563,³⁷ having gathered a company of investors to supply capital, he petitioned the Queen successfully for the right to attempt reclamation of some two thousand acres in the parishes of Lesnes, Erith, and Plumstead in Kent along the lower Thames. This area had formerly been capable of maintaining many inhabitants, but the floods of the past thirty years had devastated it and impeded river navigation with silt. Aconcio indicated that once before, presumably on the Continent, he had had something to do with such a reclamation, but nothing was accomplished because of quarreling among the former owners of the now-useless land. He therefore requested that all rights of previous possession be declared void by reason of abandonment, and that he and his company be given half of any land that they could reclaim.

³³ Lynn White, jr., "Tibet, India, and Malaya as Sources of Western Medieval Technology," *American Historical Review*, LXV (Apr. 1960), 519.

³⁴ Vittorio Zonca, *Nova teatro di machine et edificii* (Padua, 1607), 115; next by Heinrich Zeising, *Theatrum machinarum* (6 vols., Leipzig, 1614-21), II, 51-52, fig. 18.

³⁵ *Calendar of Patent Rolls, Elizabeth, 1558-1560* (London, 1939), 254.

³⁶ *Calendar of Patent Rolls, Elizabeth, 1560-1563* (London, 1948), 101.

³⁷ *Calendar of State Papers, Domestic, Elizabeth, 1601-1603, with Addenda, 1547-1565* (London, 1870), 538-39; see the interpretation of this difficult text by Delio Cantimori in O'Malley, Aconcio, 47; *Calendar of State Papers, Foreign Series, Elizabeth, 1563* (London, 1869), 92. For an excellent map of the Plumstead, Erith, and Lesnes marshes, but without mention of Aconcio, see F. C. J. Spurrell, "Early Sites and Embankments on the Margins of the Thames Estuary," *Archaeological Journal*, XLII (Sept. 1885), 302.

On February 26, 1563,³⁸ a bill to this effect was introduced in Parliament and soon passed. By June 23, 1563,³⁹ Aconcio was collecting workmen, lighters, wagons, and brush for the new embankments at Plumstead. But while at first things went well, and some six hundred acres were reclaimed, new floods shortly swept over the lowlands. Returning to the attack, the company regained the six hundred acres, but in 1566 Aconcio turned over his share to Castiglione and his other partners in an effort to give them some compensation for the five thousand pounds they had invested in the project.⁴⁰ While only partially successful, Aconcio's presumed demonstration of new reclamation methods worked out in northern Italy appears to have stimulated a great wave of English embanking and draining starting in the next decade.⁴¹

Aconcio was, after all, primarily an expert in fortification, as Throckmorton had written to Cecil in August 1559. It must therefore have exasperated him to have no part, at first, in the biggest English fortification project of the Renaissance: at Berwick upon Tweed.

Berwick was the key fortress on the English side of the Scottish border. As the peril of French invasion through Scotland grew, concern over Berwick's defenses mounted. In the last year of Mary's reign—by January 1558⁴²—the strengthening of them had been placed in charge of Sir Richard Lee (the first English engineer to be knighted),⁴³ presumably because his association in 1545-1546 with Antonio da Bergamo and Gian Tomaso Scala⁴⁴ had made him the chief insular expert in such matters. Lee quickly produced two alternate proposals for Berwick's refortification. Each involved Italian bastions:⁴⁵ the first enclosed a smaller area and had smaller bastions; the second, which was adopted as the basic plan, embraced a larger area and involved larger bastions. Construction started at once. Lee was not stupid; a very advanced technical aspect of his plan was to carry the masonry scarp only to the height of the counterscarp, so that all masonry would be protected from direct artillery fire, which would strike only the earthworks above the scarp.⁴⁶ On August 28, 1559,⁴⁷ Sir James Crofts, governor of Ber-

³⁸ O'Malley, *Aconcio*, 48-49.

³⁹ *Calendar of Patent Rolls, Elizabeth*, 1560-1563, 526.

⁴⁰ William Dugdale, *History of Imbanking and Draining of Rivers, Fens and Marshes* (London, 1792), 64.

⁴¹ See H. C. Darby, *The Draining of the Fens* (Cambridge, Eng., 1940), *passim*, which, however, does not mention Aconcio's work.

⁴² MacIvor, "Elizabethan Fortification," 67.

⁴³ John Harvey, *English Mediaeval Architects* (London, 1955), 161-62.

⁴⁴ See note 18, above.

⁴⁵ MacIvor, "Elizabethan Fortification," 67, plate XXIII, figs. 2-5.

⁴⁶ *Ibid.*, 72-73.

⁴⁷ *Calendar of State Papers, Foreign, Elizabeth*, 1558-1559, 504.

wick, and Sir Ralph Sadler wrote to their new Queen saying that Lee was on his way to discuss the Berwick problem with her: the works, although costly, were impressive and could be made very strong. In their unfinished state, however, the town was as vulnerable as before the changes were made. They appealed for haste and urged an appropriation for the coming year equal to that for the previous two years.

Neither Elizabeth nor Cecil enjoyed spending on this scale, and they doubtless wondered whether their English engineers were capable of using such funds to best effect; it was in these months that Cecil was reading books on architecture and negotiating with Portinari and Aconcio. In 1557 a Venetian ambassador had observed that the Flemish fortresses "son fortificati all'antica, et racconci alla moderna."⁴⁸ Surely those on the western side of the Channel could not be left obsolete. Yet, in reaction against Mary's reign, xenophobia was rampant in England, and Elizabeth's new government was reluctant to put foreigners in positions of command over the Queen's subjects.

When Portinari at last managed to jump the narrow seas in the summer of 1560,⁴⁹ he quickly met difficulty, which he reported to the Council in October.⁵⁰ He had been sent to Berwick and found defects in Lee's work. Portinari prepared three different plans for Berwick, the last of which was approved by the Duke of Norfolk. During his absence from Berwick, one of his plans was stolen from a locked coffer, presumably by Lee.

It would seem that neither Elizabeth nor Cecil was willing at that moment to support an Italian against the leading English military engineer. There is silence about Portinari until late in 1562 when he was sent to Le Havre to look at Lee's operation in refortifying it against the expected French attack.⁵¹ He stayed there as Surveyor of the Works, but on June 29, 1563, the Council complained⁵² that despite his diligence Portinari was greatly hindered because the laborers esteemed him so little that he could not get any of them to do what he commanded. Animosity toward Italian experts was not a matter of social class.

The Queen, as well as Cecil, continued troubled about the adequacy of the very costly Berwick refortification. On May 25, 1564,⁵³ she wrote to Bedford, governor of Berwick: she was disturbed by Portinari's criticisms of

⁴⁸ Louis Prosper Gachard, *Relations des ambassadeurs vénitiens sur Charles-Quint et Philippe II* (Brussels, 1855), 84, n. 3.

⁴⁹ See note 21, above.

⁵⁰ *Calendar of State Papers, Foreign, Elizabeth, 1560-1561*, 351-52.

⁵¹ *Calendar of State Papers, Foreign Series, Elizabeth, 1562* (London, 1867), 336, 504, 510, 513, 559, 573; *Calendar of State Papers, Foreign, Elizabeth, 1563*, 48, 372, 373, 396, 404, 425, 431-32.

⁵² *Ibid.*, 433.

⁵³ *Calendar of State Papers, Foreign Series, Elizabeth, 1564-1565* (London, 1870), 139.

Lee's plan, had examined Portinari's plan, and was impressed by the bastions and the casements in the flankers for scouring the ditch and walls with fire. She was therefore setting up a commission composed of Lee, Portinari, and William Pelham to consult on the defenses of Berwick and report to her.

Then Cecil, or the Queen, had a second thought: a commission of two Englishmen and a single Italian would inevitably support Lee's ideas. In a letter sent the same day⁵⁴ informing Pelham of his appointment, a fourth member was added to the commission: Aconcio. The expense accounts,⁵⁵ however, indicate that Portinari had seniority over Aconcio; for his trip to Berwick the latter received £20 as compared with Portinari's £27, 13s.4d.

The commission's report is dated June 11, 1564,⁵⁶ and represents a confrontation between men who believed themselves to be up to date and men who knew that their opponents were slightly out of date. Lee and Pelham wished to continue the existing plan, with minor improvements, whereas Portinari and Aconcio recommended drastic and costly change. A new wall must be built eastward from the bulwark beneath Cowgate to the sea. The lower town should be included in the defenses. Moreover, the Italians considered the entries to the casemates too narrow to admit cannon, whereas Lee and Pelham thought them wide enough. Four days later⁵⁷ the Earl of Bedford desperately asked the Queen to send "some persons of credit hither" to judge the merits of the conflicting plans, and simultaneously⁵⁸ wrote a soothing letter to Cecil praising the conduct of the commission. By the same mail Portinari sent duplicate personal reports to the Queen and to the Privy Council,⁵⁹ while Lee complained in still another missive that Portinari's plans were too expensive and submitted two of his own.⁶⁰

Aconcio held his fire for a week, perhaps suspecting that the court would be as confused as Bedford and might welcome a new voice when the first shouting had begun to subside. On June 22 he sent off his own memorandum: six pages in Italian.⁶¹ He elaborated the importance of fortifying the lower town and the eastern side of the peninsula toward the sea and specifically recommended one of Portinari's plans for this. He sharply criticized the inadequacy of the current schemes for defending the bridge over

⁵⁴ *Ibid.*, 140.

⁵⁵ *Acts of the Privy Council of England, New Series* (32 vols., London, 1890-1907), VII, 145-46.

⁵⁶ *Calendar of State Papers, Foreign, Elizabeth, 1564-1565*, 154.

⁵⁷ *Ibid.*, 155.

⁵⁸ *Ibid.*, 156.

⁵⁹ *Ibid.*

⁶⁰ *Ibid.*

⁶¹ *Ibid.*, 163; text in Rossi, *Aconcio*, 22-23, n. 40, and in O'Malley, *Aconcio*, 52-55. A sketch map by Aconcio, which presumably accompanied this memorandum, is preserved at Hatfield House; see MacIvor, "Elizabethan Fortification," 84, plate XXVa.

the Tweed, which was the reason for Berwick's existence. In the matter of the size of casemates, he insisted that they were being built too narrow, for all experience taught that they must be wide enough "*che un cannone possa esser commodamente condotto entro e fuori sopra le sue rote.*" The present walls, moreover, were so high that they were needlessly vulnerable to artillery, and they were not adequately sloped: a good wall retreats a foot for every four vertical feet, and it rests on earthworks behind it.

Up to this point Aconcio was attacking Lee, but at the end, without mentioning Portinari, he seemed to disagree also with his fellow Italian. There was debate as to the extent to which earth reinforced with beams and fascines should be used at Berwick. Aconcio said that such construction was satisfactory for relatively temporary fortifications in a war aiming at the permanent conquest of a region bit by bit, as with the Turks in Hungary or the French in Piedmont. Since Portinari had built forts for the French in Piedmont,⁶² we may guess that he had backed Lee's proposal, to some extent, for such construction at Berwick. Aconcio, however, opposed it; defenses on the stable frontier of a kingdom should be built of masonry to endure, since when the beams of earthworks rot, the expense of moving the earth to replace them more than eats up any initial saving. Here Aconcio touched upon one of the major controversies among the engineers of his time.

The British Museum, Cottonian MS Titus F. XIII, fols. 232^r-234^v, contains a second memorandum by Aconcio on Berwick, hitherto unidentified because it is listed as anonymous.⁶³ This time it is written in Latin, and the literary format is more elaborate than that of the first; it was clearly designed for the Queen's eye. Since the discussion is somewhat further removed from the commission's report of June 11, 1564, it is probably a bit later than the Italian memorandum.

Indeed, Aconcio disdained (fol. 234^r) even to discuss Lee's plan: "*praetermitto enim nunc quam id fuisset ineptum.*" What he himself proposed was that one side of the city be defended with a very thick wall, slightly inclined, and the other side with a thinner wall resting more on its earthen backing than upon its foundations. Against those who claimed that this latter would not stand, he asserted that such construction had been entirely successful at Milan, Pesaro, and many other fortresses. He confessed, however, that they were built of brick, which sustains bombardment better than stone (fol. 234^v). The Italians, nevertheless, used either brick or stone for such a wall, merely making it a bit thicker when stone was employed.

⁶² See note 20, above.

⁶³ While in an Italianate hand, this is not the original, since the name "Oligati" is misspelled "Oligati."

To anyone charging that he was sacrificing security to economy in his recommendation, Aconcio replied: "This kind of wall was invented not so much to reduce costs as because it is far less damaged by cannon."⁶⁴ Quite apart from the cost of a wall constructed with big stones "et admodum duris," it was exactly this sort of wall that artillery reduced most easily. Thus Aconcio proposed that if there were too little wood around Berwick to bake bricks, they be fired elsewhere and brought in by ship. He had, however, another idea: "Not far from the town I noticed a certain kind of stones, and if (as I hope) there may be enough of them, there will be little need of bricks. They are easily split, quite flat, and not too thick, so they can conveniently serve for masonry and make a fabric very like one of bricks; and this can be done at much less cost than in ashlar."⁶⁵

Finally, Aconcio returned to the proposal that the masonry wall be raised no more than seventeen feet and that earthworks reinforced with beams and fascines be set above it. He could not advise it; to be sure, at various places in Germany he had seen such construction, but "neque Germani tantum habent harum rerum usum, ut eorum quenquam movere debeat autoritas." He repeated the adverse argument of his earlier memorandum and insisted that such methods should be used only in the face of threat of immediate invasion, or when it was hoped that expanding conquest would lead to the abandonment of a fortification.

The Elizabethan defenses of Berwick survive very nearly intact. They are a major monument of sixteenth-century military architecture, since when they were built they were probably the most spectacular example in Northern Europe of the new Italian manner of fortification. Recent excavations have begun to show in detail how Portinari's and Aconcio's criticism shaped the work:⁶⁶ defenses were built eastward to the sea; bastions and flankers were enlarged and modified; the batter of scarps was increased; casemates were made wider to permit the maneuvering of cannon. By 1558 Lee had already learned something of the language of the new Italian military art; at Berwick his Italian colleagues greatly improved his accent.

In his Latin memorandum, only the concluding and shorter part was devoted to Berwick. The rest was the mellow reflection of an aging man (Aconcio died, as has been said, in 1566 or 1567) upon his life and profession.

⁶⁴ "Addo etiam talem murorum rationem excogitatam fuisse, non tantum ad minuendas impensas: sed multo magis quia bellicis machinis non tantopere laedantur."

⁶⁵ "Non procul ab oppido animadverti quoddam esse lapidum genus, quorum si satis magna esset copia (ut spero esse) lateribus non magnopere esset opus. fissiles enim sunt: ideoque plani, et parum spissi. ut in opere instar lapidum commodissime iacere possint: opusque facere quod ad lateritium proxime accedat: fieri autem possit longe minore sumptu, quam quadratis lapidibus."

⁶⁶ MacIvor, "Elizabethan Fortification," 87-93.

The military engineer, he said (fols. 232^r-233^r), found it hard to become expert because he had no adequate controls. Fortifying a city was a long task, and fortified cities were rarely attacked. When they fell they might do so not because their defenses were defective but by reason of cowardice, treason, or lack of supplies; the true cause was often hard to ascertain. Thus an engineer did not often have the opportunity to profit by his earlier mistakes when he was designing new works. For this reason it was particularly necessary for a military engineer to share the experience of as many others as possible, and not try to be his own Mars. The art of fortification had reached its peak in Lombardy, and especially in the area of Milan, because not only Italians but also Spaniards, French, and Germans had so often been both besiegers and besieged in that region. At this point Aconcio became autobiographical (fol. 233^r, bottom):

I lived nearly seven years with a certain Italian Count, Francesco Landriano by name, at the court of the Emperor Charles V and of his son King Philip, but then also with the army in what was Hither Gaul to the Romans; today they call it Lombardy and Piedmont. From boyhood almost to old age he had been much involved in military matters and was Charles V's adviser (fol. 233^v) for war and for all the other things which are involved in military science. Thus he was an expert on the design of fortifications; yet such was his nature that he was not ashamed to learn from whomever he could. So I could not fail to learn many things every day, since, because of the great authority which he wielded, many captains, old soldiers of every rank, and military engineers daily flocked to him, and he enjoyed nothing more than talk of military affairs, and I was almost always present at that sort of discussion, the more so since if he had picked up from anyone anything concerning the art of defenses, he would order me to write it down.

Moreover, my mind was long since vexed with plans to flee whither I might freely profess the Gospel, and I thought it possible that if I might learn this art it would thereafter give me a living. So I set myself in earnest to learning it. Whenever I had a chance I carefully questioned everyone about what they had tried to do, and what they admired or condemned in every fortress. All these things I held in my memory, and whenever I came where there was a fortification I observed everything carefully. Finally when (as I have said) I was with the army in Hither Gaul in company with this same Count, I had occasion to see nearly all the defended sites in that region, and even shared in throwing up emergency fortifications for some towns.

Later, when the Cardinal of Trent was Viceroy in Milan and I was his secretary, I enjoyed the friendship of a man who had long held undisputed first place among Charles V's military engineers, Giovanni Maria Olgiati. No one has ever been kinder to me than he, or swifter to explain anything concerned with that art; and when he got to know me and tested me—and here let me offer him thanks—on whatever matters I questioned him (and there was scarcely any aspect of the art on which I did not question him), he freely and quickly gave answer. At that time, moreover, we had before our eyes some of his works, both early and more recent ones, nor was he reluctant to tell us what blunders he had

found in any of his own constructions either by himself or spurred by those who had shown that by some test they had learned something better.⁶⁷

All biographers of Aconcio⁶⁸ have been puzzled by lack of evidence about his career prior to his flight from Milan, and about the origin of the reputation as an engineer which won him such important contracts for reclamation and fortification after he went to England. Our new document tells us something and may serve to guide other scholars to unexploited archives.

We had known that Aconcio had friends in the court at Vienna. Now we have his assertion that for nearly seven years (from 1549-1550⁶⁹ until 1556 when he became Cardinal Madruzzo's secretary) he was in the retinue of Count Francesco Landriano, largely at the court of Charles V and Philip II. Landriano has not attracted the attention of modern scholars; yet he was a major figure of the Empire in his day. He was reared at the court of Francesco Maria della Rovere, duke of Urbino, in company with the latter's heir Guidobaldo. As a young man he entered the service of Charles V. In July 1541, after imperial soldiers had murdered the French ambassador to Turkey near Pavia, Landriano was sent to Francis I to attempt to explain the disaster. On April 24, 1547, at the climax of the Schmalkaldic War, Charles V chose Landriano to lead a small party of horsemen to find a ford

⁶⁷ "Vixi ad septem annos apud quendam Italum Comitem cui nomen erat Franciscus Landriano, maxima ex parte in curia Caroli V Imperatoris, et Regis Philippi filii, sed demum etiam militiae in ea quae fuit Romanis citerior Gallia: hodie Lombardiam et Pedemontem appellant. Fuerat is in bellicarum rerum tractatione a pueritia pene ad senectam usque valde exercitatus. eratque Caroli V rei bellicae consiliarius (fol. 233^v), ut aliarum rerum omnium quae ad militarem scientiam pertinerent, ita rationis muniendi erat studiosissimus. et eo erat ingenio: ut non duceret pudori a quocumque posset, discere, ita cum propter magnam qua pollebat, auctoritatem multi quotidie centuriones, omniumque ordinum veterani milites et militares architecti ad eum confluerent, vixque aliis delectaretur sermonibus quam qui essent de rebus bellicis, eiusmodi autem sermonibus ego fere semper interesssem, quin etiam praecipue si quid quod ad muniendi artem pertineret ex aliquo didicisset, me id iuberet annotare non poteram non multa quotidie addiscere.

"Praeterea cum pro iis consiliis quae iam tum animo agitabam, videlicet de migrando ubi libere profiteri possem Evangelium, fieri posse cogitarem ut si hanc artem didicissem, mihi aliquando victum suppeditaret: serio ad eam adipiscendam animum adieci. ita quacumque dabatur opportunitas, interrogabam sedulo quid quisque expertus esset: quid in quoque oppido laudarent, quid repraehendarent: eaque omnia memoriae mandabam. quocumque veniebam ubi aliquid esset munitionis: observabam omnia diligenter. tandem cum militia (ut dixi) apud eundem essem Comitem in citeriore Gallia: omnia propemodum quae sunt in illa regione munita loca, videre mihi contigit: atque etiam celeri aliquot oppidorum munitioni interesse.

"Postremo cum Cardinalis Tridentinus in Mediolanensi ditione praelegem ageret, meque uteretur secretario: fuit mihi amicitia cum eo qui inter Caroli V militares architectos diutissime absque controversia primum locum obtinuerat. Jo. Maria Oligatus [*sic*] nomen ei erat. quo quidem nemo unquam mihi visus est melius, ac promptius cuiusque rei ad eam artem pertinentis rationem reddere. cumque intelligeret atque etiam expertus esset me tum eo esse loco ut ei gratiam referre possem: quibuscumque de rebus illum interrogarem (vix autem ullum est artis caput de quo illum non interrogaverim) liberaliter ac prompte mihi respondebat. tum autem etiam erant ante oculos eius opera et vetustiora aliqua, et recentia. neque pudebat eum fateri quid erroris in quoque suorum operum animadvertisset, sive sua sponte, sive admonitus ab iis qui aliquo experimento melius aliquid se didicisse ostenderent."

⁶⁸ E.g., O'Malley, *Aconcio*, 10-13, 47-48.

⁶⁹ He was still in Trent in 1549; see *ibid.*, 11.

across the Elbe at Mühlberg. He succeeded, established an imperial foothold at the further shore, and Charles won his greatest victory. For judging Aconcio's professional connections it should be noted that Landriano's party fording the Elbe included the famous engineer Francesco Ferretti, whose *Dell'osservanza militare* (Venice, 1568; 2d ed., 1576) was well known in later years. Presumably the latter was often thereafter in Landriano's entourage and would therefore have been known to Aconcio; for in August 1557 Ferretti was sent to London by the Duke of Urbino to plead successfully with Philip II in the case of the Count of Landriano who, for reasons unknown to us, had been imprisoned in the castle of Milan.⁷⁰ One wonders whether Aconcio's flight in June 1557 may have brought suspicion of crypto-Protestantism upon his patron. The hypothesis is strengthened by the fact that Landriano, presumably to win orthodox favor, turned over to the clergy a heretical letter written on October 16, 1557, to the Marquis of Pescara by Francesco Betti, Aconcio's friend and fellow refugee in Switzerland.⁷¹ The episode did not injure Landriano's career; in the spring of 1563 he was in Rome representing Pope Pius IV in dealing with Philip II's ambassadors to the Holy See to bring pressure, through Spain, upon the German Habsburgs regarding papal confirmation of Maximilian's election as king of the Germans.⁷² In 1566 Girolamo Ruscelli spoke of him as "cre-scendo in consideratione di tutti i primi Principi d'Europa."⁷³

What of Olgiati whom Aconcio describes so movingly, and whom he ranks as by all odds the best of Charles V's military engineers? It is indicative of the rudimentary state of the history of technology that we know little about him, as yet. He was a native of Milan who by 1537 was designing a new fortress for Savona, in 1547 was working on the defenses of Vienna, about 1550-1552 was at Siena and perhaps briefly on the Balearic Islands, in 1553 went to Flanders, in 1554 was back in Italy redesigning Albenga, and in 1555, together with the engineer Sigismondo da Pratovecchio, was completing the fortification of Murnay west of Halicz, in what was then northern Hungary.⁷⁴ On January 17, 1556, by a *motu proprio* given at Brussels,

⁷⁰ Carlo Promis, "Gl'ingegneri militari della Marca d'Ancona," *Miscellanea di storia italiana*, VI (1865), 271, see also 267; Karl Brandi, *The Emperor Charles V* (London, 1939), 468.

⁷¹ O'Malley, *Aconcio*, 66. Landriano was still in good standing on March 2, 1557, when Philip gave orders for a payment to him at Milan. (See Antonio Monti, "Filippo II e il Card. Cristoforo Madruzzo Governatore di Milano (1556-1557)," *Nuova rivista storica*, VIII [No. 2, 1924], 153.)

⁷² Robert Holtzmann, *Kaiser Maximilian II. bis zu seiner Thronbesteigung (1527-1564)* (Berlin, 1903), 452, n. 1, 470.

⁷³ Girolamo Ruscelli, *Le imprese illustri* (3d ed., Venice, 1580), 208, see also 206-207. The first edition was published in 1566.

⁷⁴ Carlo Promis, "Gl'ingegneri militari che operarono o scrissero in Piemonte dall'anno MCCC all'anno MDCL," *Miscellanea di storia italiana*, XII (1871), 515-22; L. A. Maggiorotti, *Architetti e architettura militari* (2 vols., Rome, 1933-36), II, 101, 340.

Philip II appointed Olgiati to design a bastioned fortification for Milan⁷⁵—one of the most important commissions of the century. On April 13, 1557, in London, Philip was trying to decide what should be done about Olgiati's recommendations for the crucially important fortification of Tortona.⁷⁶ In 1560 Olgiati was at work on the fabric of Milan cathedral.⁷⁷ Aconcio's admiration was not misplaced.

Having sat at the feet of such a Gamaliel, and having recorded, at the Count's request, the discussions of the *militares architecti* surrounding Landriano, it is not surprising that the refugee Aconcio hoped (as he tells us) to make his living in exile practicing the Italian art of defenses, and also that he should have undertaken to produce a book on the subject. We know from his other writings that he composed it first in Italian and then put it into Latin,⁷⁸ presumably after he reached England where Latin was more widely read. Since the seventeenth century there have been assertions that this latter was published posthumously at Geneva in 1585 under the title *Ars muniendorum oppidorum*.⁷⁹ No one, however, has yet found a copy or has turned up a citation from it in a later writer; it is generally regarded as a bibliographical "ghost." Nevertheless, this ghost may eventually assume ectoplasm if not flesh. None of the printed references to the Geneva 1585 edition indicate its format, but Biblioteca Nazionale, Florence, MS Palat. 995, an anonymous *Essay de Bibliothèque Militaire* (the latest item of which was published in 1744), on page 9, states that it was in octavo.

On his arrival in England, and probably later as well, Aconcio gave copies of his treatise on fortification to influential persons. In Cottonian MS Titus F. XIII, fol. 234^r, he protested that his criticisms of Lee's plans for Berwick were not based on a wish to inflate his reputation by controversy, but rather that they reflected all that he had previously taught and said: "Clear witness of this is the fact that when first I came to England I circulated an *Ars muniendi* written by me. The things which I am now saying are in agreement with the contents of that treatise, as those who have copies can testify."⁸⁰ The destruction of manuscripts in England since the days of

⁷⁵ Francesco Cazzamini Mussi, *Milano durante la dominazione spagnuola (1525-1706)* (Milan, 1947), 169.

⁷⁶ Monti, "Filippo II," 155.

⁷⁷ Cazzamini Mussi, *Milano*, 185.

⁷⁸ O'Malley, *Aconcio*, 28.

⁷⁹ M. J. D. Cockle, *Bibliography of Military Books up to 1642* (2d ed., London, 1957), 203; Horst de la Croix, "The Literature on Fortification in Renaissance Italy," *Technology and Culture*, IV (Winter 1963), 48.

⁸⁰ "Certissimum esse potest testimonium, quod quum primum in Angliam venissem, muniendi artem a me conscriptam ostenderim. quodque ea quae nunc dico iis sint consentanea quae tractatione illa continentur. id quod testari possunt ii qui eius exempla habent."

Elizabeth I has not been so great as to deprive us of hope that eventually one of these copies may be found.⁸¹

It is not only the later twentieth century that stands amazed at the many aspects of the vitality of England under the Virgin Queen; in 1655 Samuel Hartlib looked back and noted that "In Queen *Elizabeth's* dayes, *Ingenuities* [and] *Curiosities* . . . began to take place, and then *Salt Marshes* began to be fenced from the Seas."⁸² Much of the yeast for this ferment came from abroad. Aconcio's contribution was greater than has been recognized, and his works are the more astonishing because they were accomplished in only seven years. He was the most lucid, and probably the most influential, of the early spokesmen for religious tolerance; this has long been recognized. But we must add other laurels. He was the first in England to reconsider the theory of history. He was the first in England to announce the modern concept of grants of patents to inventors in the public interest. He launched what was probably the first English reclamation project to use the new techniques developed in Italy. Finally, at Berwick he and Portinari did much to introduce modern military engineering to England. He was supremely a "man of the Renaissance" as well as of the Reformation

⁸¹ Cecil would certainly have had a copy, and many of his papers are at Hatfield House. Salisbury MS 210.6 (*Calendar of the Manuscripts . . . at Hatfield House*, XIV, 307), however, described as a fragmentary anonymous Italian treatise on fortification of Elizabethan date, disappoints us. It is, in fact, two fragments. The first (pp. 1-20) deals with the art of fortification, but does not parallel in substance or style the known writings of Aconcio. The second fragment (pp. 21-44), which may or may not have been written by the penman or author of the first, is an essay on the French Wars of Religion written after 1577. The Marquess of Salisbury and Miss Clare Talbot, librarian of Hatfield House, permitted photographs to be taken.

⁸² Samuel Hartlib, *Legacy of Husbandry* (London, 1655), 41.

The Strange Stillbirth of the Whig Party

LYNN L. MARSHALL*

THE Whig party's peculiar birth cannot properly be understood with reference to politics only. In fact, its birth seems to have been integral to concurrent changes in American social organization in a very general sense. Let us entertain the hypothesis that the most significant developments in early nineteenth-century America involved not elevated political ideology (as, for example, states' rights versus national power and laissez faire versus state regulation), but rather changes in the ways Americans organized themselves to solve immediate problems of all sorts, whether public or private. Whig birth coincided with the crest of a ground swell of social change that would shortly reorganize American life around a proliferating series of specialized, large-scale organizations, flexible, functional, and impersonal.

The key element in the formation of the Whig party was party organization, not ideology. There seems sufficient reason to assume that Whig ideology, in early infancy at least, limited itself to opposition to "executive usurpation," the negative issue implied in its choice of name and the focus of its electioneering efforts throughout the mid-1830's.¹ One can hardly find another common ground between John C. Calhoun, prince of nullification, and nationalists like Henry Clay and Daniel Webster, all of whom joined to establish the party. This issue expressed a reaction to threatening changes in party organization and the role of political leadership. How did the Whig party, then, differ from the Jackson party in organizational structure? How did each relate to the structure of society at large? The conditions of Whig birth, seen in this light, may offer some enlightenment on that much-disputed entity, "Jacksonian Democracy."

Part of the obscurity concerning Whig origins has involved its precise date of birth. It has usually been placed somewhere in 1833 or 1834.² The

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¹ The Whig name of course had originated long before in the English parliamentary party opposing usurpations of the king. American colonists during the Revolution had borrowed the name to indicate a similar opposition to the usurpations of George III.

² See, e.g., Charles M. Wiltse, *John C. Calhoun: Nullifier, 1829-1839* (New York, 1949), 223; Charles G. Sellers, Jr., *James K. Polk: Jacksonian, 1795-1843* (Princeton, N. J., 1957), 212-13; Glyndon G. Van Deusen, *The Jacksonian Era, 1828-1848* (New York, 1959), 96. The traditional view has attempted to make sense of the Whig party while maintaining primary focus on formal, explicit, ideological main currents running through the full span of

name "Whig" was not used formally to designate the party until 1834. If, however, the alignment around the issue of executive usurpation is taken as the effective birth, it may be fixed with precision at a considerably earlier date. It came in July 1832, in direct and explicit reaction to Jackson's veto of the bill rechartering the national bank. Although the anti-Jackson party continued to call itself National Republican through the campaign of 1832, it abruptly and consciously became proto-Whig with the veto message.

On July 11, 1832, Webster rose in the Senate to denounce the Bank veto at great length, with all the force of his inimitable voice and commanding presence. Only the day before Jackson had sent his veto message to the Senate, and at the first possible moment Webster sprang to the attack. Webster paid scant attention to the arguments put forth in the veto message. He simply disdained it. He ignored, furthermore, what was ostensibly the principal doctrine of his National Republican party: nationalism versus doctrine states' rights. At one point he even noted that the veto's main argument was "little compliment to State sovereignty"—as indeed it was, despite rhetorical flourishes to the contrary. Webster focused instead on "executive usurpation."

According to the doctrines set forth by the President, although Congress may have passed a law, and although the Supreme Court may have pronounced it constitutional, yet it is, nevertheless, no law at all, if he, in his good pleasure, sees fit to deny it effect; in other words to repeal and annul it.³

With this veto, concluded Webster, Jackson had effectively proposed a "pure despotism," and announced, like Louis XIV, "I AM THE STATE."⁴

On the following day, Henry Clay, the National Republican presidential nominee, delivered similar expressions to his Senate colleagues.⁵ This double-barreled display was the more impressive because Webster, Clay, and the other pro-Bank oratorical giants had participated sparingly in the long debate that preceded passage of the bill, knowing that they had sufficient votes to ensure passage. Now, however, they unleashed their full power. Their speeches, moreover, in length, polish, and fluency of delivery, suggested considerable preparation.

Up to that time Webster, Clay, and party had not agitated executive usurpation, but for the remainder of the 1832 campaign it became the main party issue. The *National Intelligencer*, their principal party newspaper,

American experience, rather than on more explicit, functional ideology. For an example of this, see *id.*, "Some Aspects of Whig Thought and Theory in the Jacksonian Period," *American Historical Review*, LXIII (Jan. 1958), 305-22.

³ "Speech of Mr. Webster," *Washington National Intelligencer*, Sept. 22, 1832.

⁴ *Ibid.*

⁵ "Speech of Mr. Clay," *Niles' Weekly Register*, Aug. 11, 1832.

hammered away at it in long series of editorials, calling Jackson a "monarch," "KING OF KINGS," and even "DICTATOR." This campaign originated the "King Andrew" cartoon that would become the Whigs' stock in trade for the next few years. The Bank veto message, the *Intelligencer* warned, had frankly announced "downright Tory doctrines."⁶

By August the accession of Calhoun completed the basic Webster-Clay-Calhoun Whig alignment. Just three weeks after the Bank veto, the *United States Telegraph*, Calhoun's organ in Washington, suddenly began to say pleasant things about Clay. Soon the *Telegraph* printed Clay's Bank veto speech and joined the opposition cacophony about "Executive tyranny."⁷ It was odd, on the face of it, that this organ of nullification and ostensible enemy of the Bank should thus abruptly espouse the great hero of tariff and Bank, and strange too that the Webster-Clay-Calhoun alignment would remain intact through the subsequent nullification crisis. A more compelling issue than simple states' rights was at work here.

Prior to July 1832 the National Republicans, an alignment of leaders with strong local identifications, like Clay's to Kentucky and Webster's to Massachusetts, had bent their efforts mainly to showing themselves "national." They had sought to identify themselves with the Bank, internal improvements, and especially the tariff. Executive usurpation had not concerned them. Clay had devoted his campaign efforts mainly to the tariff; he delivered a great tariff speech before the Senate in February, which the party promptly printed and broadcast over the country for political effect.⁸ The National Republican "platform"—called the "Address of the National Republican Convention to the People of the United States"—had likewise stressed such "national" issues as the tariff. It too had failed to feature a theme of executive usurpation.⁹

When Webster initiated the charge of executive usurpation so suddenly at the time of the veto, he presented an obviously trumped-up case. The Constitution gave the President an absolute right to veto legislation, *whatever* his reasons or lack of them. The Bank veto message clearly differentiated between the President's legislative and executive functions, just as did the Constitution. Webster, however, advanced the entirely specious argument that Jackson repealed and annulled an established institution, not a mere bill.

⁶ Washington *National Intelligencer*, Sept. 22, 1832; "Review of the Veto," *ibid.*, Oct. 4-18, 1832, in seven numbers; Frances Kemble, *Journal* (2 vols., Philadelphia, 1835), I, 141.

⁷ Washington *United States Telegraph*, July 23, Aug. 2, 6, 1832. Calhoun could not bring himself to announce personal, public support of Clay, but the *Telegraph* made his position perfectly clear. Cf. Wiltse, *Calhoun: Nullifier*, 141.

⁸ *Niles' Weekly Register*, Feb. 2, 3, 6, Mar. 3, 10, 1832; Clay to Francis Brooke, Mar. 17, 1832, *Works of Henry Clay: Comprising His Life, Correspondence and Speeches*, ed. Calvin Colton (10 vols., New York, 1904), IV, 329.

⁹ *Niles' Weekly Register*, Dec. 24, 1831.

Jackson, of course, had sought neither to repeal the original charter of the Bank nor to touch the existing institution in any way. For a nationalist, Webster certainly took a novel position. From the outset nationalists had demanded a powerful executive, and, in fact, the plan of the Bank of the United States itself had originated, in 1791, in the executive department. Clearly, something other than the question of nationalism was involved here.

The proto-Whigs calculated and literally forced the issue of executive usurpation, emasculating their ideology in the process. The Jacksonians had struggled to prevent the Bank issue from coming up in that session of Congress. Because Jackson's re-election in 1832 was almost assured without the Bank issue, they would have preferred to avoid anything so potentially dangerous, for Jackson had made known his intent to veto a recharter that did not involve basic changes in the character of the Bank. "... If Jackson is to be believed," Clay snidely commented, "he will veto it."¹⁰ The proto-Whigs had to force the veto in order that they might raise their trumped-up issue.¹¹ Why should they have done it? Webster and Clay were shrewd political leaders, not accustomed to raise weak or meaningless issues in the midst of presidential campaigns. There was something substantial behind their actions, although they avoided stating it explicitly, something important enough to throw a nationalist into the arms of a nullifier. The apparent peculiarities of the case indicated changes of a profound nature threatening the customary role of the political leader and the structure of his national political organization. The Bank itself symbolized the type of social structure for which the proto-Whigs stood, and the Jacksonian attack on it a new and challenging alternative.

The following circumstances concerning the Bank veto and the proto-Whig response to it deserve special consideration. Veto and response differed drastically in content, structure, and orientation. The one was aimed directly at voters; the other appealed to members of the political establishment. The veto sought to rally public opinion directly; the replies appealed to the authority of constituted leaders. The Jackson party was immediately voter oriented, and the proto-Whigs, leader oriented. This distinction underlay

¹⁰ Clay to Brooke, June 29, 1832, *Works of Clay*, ed. Colton, IV, 340.

¹¹ The issue of executive usurpation was strongly voiced in the address of the New York Antimasonic party convention, also timed to coincide almost perfectly with the Bank veto. At that convention prominent National Republicans were placed on the Antimasonic party electoral slate, in spite of their support for the well-known Mason, Clay. The Antimasonic party, meanwhile, continued ostensibly pledged to their own presidential candidate, William Wirt. Wirt represented, incidentally, a classic proto-Whig stance. The Jackson administration, he said, was a "millennium of minnows." Following the Bank veto he wrote: "According to General Jackson's principles, our government is a despotism. His veto doctrines, as illustrated by his practice, virtually annihilate both Congress and the Supreme Court." (Wirt to Judge [Dabney] Carr, Oct. 25, 1832, John P. Kennedy, *Memoirs of the Life of William Wirt* [2 vols., Philadelphia, 1856], II, 328.)

Ralph Waldo Emerson's epigrammatic characterization of the two parties: "I should say that one has the best cause, and the other contains the best men."¹²

The Bank veto speeches of Webster and Clay addressed fellow political leaders, in fact and in intent. Their style, their choice of language, and the arguments they advanced were not designed for popular consumption. These proto-Whigs sought to show that Jackson had infringed upon the accustomed powers of established, locally based political leaders. They could and did give scant attention to the arguments in the veto message, and none at all to its gist—its rejection of the use of government power to support exclusive private profit-making ventures by a pre-existing social elite—because their chief objection to the veto was its short-circuiting of established political leaders in Congress and Court. They objected to the direct appeal of the veto message to the electorate. The message chose arguments and language with an eye to popular appeal, proposing explicitly that "a new Congress, elected in the midst of such discussion [of the dangerous and unconstitutional features of the Bank], . . . will bear to the Capitol the verdict of public opinion. . . ."¹³ Webster replied to this appeal with unconcealed disdain, "[t]he message toils through all the commonplace topics of monopoly, the right of taxation, the suffering of the poor, the arrogance of the rich, with as much painful effort, as if one, or another, or all of them, had something to do with the constitutional questions."¹⁴ The *Intelligencer* likewise coupled its usurpation argument with denunciation of the "infuriate mob" to which the message directed itself, and bitter observations on how very little faith could be put in the "virtue and intelligence" of the electorate at large.¹⁵

One of the most striking features of the veto message was indeed its direct orientation to the electorate. It spoke not to the Congress that had passed the bill, nor to the Court that had, years before in the *McCulloch v. Maryland* case, ruled a national bank constitutional; it spoke in plain language to voters. And those voters in November would overwhelmingly re-elect Jackson.

Webster and the proto-Whigs all too clearly recognized that the veto message and its direct orientation to voters represented a new approach to politics and new roles for political leaders. It was precisely this that produced their carefully planned proto-Whig alignment. The message was the product, they knew, of that informal board of advisers and organizers whom

¹² Ralph Waldo Emerson, *Complete Works of Ralph Waldo Emerson*, ed. Edward Waldo Emerson (12 vols., Boston, 1903–1906), III, 209.

¹³ *A Compilation of the Messages and Papers of the Presidents, 1789–1897*, ed. James D. Richardson (10 vols., Washington, D. C., 1899), II, 589.

¹⁴ "Speech of Mr. Webster."

¹⁵ *Washington National Intelligencer*, Aug. 3, 16, 1832.

the anti-Jackson press had begun calling the "Kitchen Cabinet." To Webster, this new brand of political leadership compared invidiously with the old. No one in the Senate supposed, he said, that Jackson had written the veto message himself. This was reprehensible enough, but "whoever may have drawn it up," the President ought at least to have required it to have "passed under the review of professional characters." The matter was one to be decided by elevated, educated, established political leaders. Constituted bodies of such men, the Congress, the Court, even the President's own cabinet, had already decided the matter. How dare "these miserable people" in the "Kitchen Cabinet" presume to argue constitutional doctrine? The proto-Whigs assumed that they need only name the member of the "Kitchen Cabinet" mainly responsible for the authorship of the message to refute the whole of it. The *Intelligencer* demanded simply: "Are the People . . . willing to have AMOS KENDALL to rule over them in the name of ANDREW JACKSON?"¹⁶

The "Kitchen Cabinet" served as an early version of what would become a national committee. It directed the establishment of an efficient national party structure, beginning with local committees headed by a county superintendent, then district managers to report to state committees, which in turn reported to the central coordinating body in Washington. It built for specialized, functional ends, rather than for such generalized purposes as reinforcing the status system of society at large. It sought to construct out of functionaries recruited without primary regard to extraneous social criteria a faceless party cadre, with well-defined lines of communication and command, designed to perform the special functions of electioneering and channeling of votes. The whole structure was firmly cemented by award of federal offices, especially local postmasterships and local offices connected with the taking of the 1830 census.¹⁷ Into this wide-ranging, efficient structure the organizers at Washington fed campaign materials calculated to appeal to the grass roots, and in the Bank veto message they provided a campaign circular

¹⁶ *Ibid.*, Sept. 4, 1832. Note Richard Longaker, "Was Jackson's Kitchen Cabinet a Cabinet?" *Mississippi Valley Historical Review*, XLIV (June 1957), 94-108, which concludes that the "Kitchen Cabinet" was not a cabinet. The author gives no proper consideration, however, to its participation in noncabinet functions.

¹⁷ See, e.g., Kendall to Francis P. Blair, Jan. 9, Mar. 10, May 24, 1829, Blair-Lee Papers, Princeton University Library. This plan for "an efficient and universal organization of our party" involved the permanent establishment of procedures used for the 1828 Jackson victory in Kentucky, of which Kendall and Blair had been chief architects. For evidence of the extensiveness of party organization on this model, and its uses, consider the remarkably efficient party reaction to the Calhoun defection in 1831. Calhoun's Washington *United States Telegraph* (Mar. 18, 25, 1831) discovered to its dismay that the Jackson party organization produced identical, simultaneous, and shrewdly construed reactions to Calhoun's movements, in such widely separated locations as Ohio, New Hampshire, Kentucky, Washington, D. C., and Tennessee; the unseen hand guiding this "active corps . . . extending from Maine to Missouri" was the "kitchen cabinet."

par excellence. The proto-Whigs publicly noted that these circulars and the organization distributing them were novel and "a matured system of electioneering."¹⁸

The *Washington Globe*, the principal Jackson newspaper, had been founded in 1830 as a fundamental part of this new party organization, and as the head of a system of coordinated presses throughout the nation. It so far surpassed its contemporaries in spirit and readability that the proto-Whigs considered its style bad taste. Much of the argument and rhetoric of the veto message had long since appeared in the Jackson press.¹⁹ The Jackson party organization saw to it that special electioneering *Globe* extras and copies of the veto message papered taverns and public places throughout the land. In the remotest counties of the West, voters who had never experienced the least contact with the Bank had begun pressing politicians with detailed inquiries about it.²⁰

Some of the most important and innovative features in Jackson party organization were in the roles of its functionaries. It introduced new and lasting types of political leaders. The fact that the party organizer and the party candidate, as redefined in the Jacksonian era, have continued with little change down to the present suggests the long-range significance of these developments. Martin Van Buren, Jackson's chosen successor in the presidency, epitomized the new type of candidate, and Kendall, whom the opposition press called "chief cook and scullion" in the "Kitchen Cabinet," the organizer. Van Buren earned the sobriquet "Little Magician" for political know-how. Wordy, smooth in delivery, practiced in traditional political rhetoric, noncommittal if possible, wise in the ways of voters, Van Buren always kept his finger on the public pulse and studiously avoided wounding anyone's feelings unnecessarily. Although intelligent and successful, he yet suffered from feelings of inferiority because he had never attended college, and he certainly never began to produce any theory of government to match those of John Adams, Alexander Hamilton, or Thomas Jefferson.²¹

Kendall was a former Kentucky newspaper editor rewarded by Jackson

¹⁸ "The Babbling Politician," *ibid.*, Aug. 2, 1832.

¹⁹ Kendall to Blair, Jan. 9, Mar. 10, May 24, 1829, Aug. 22, Oct. 2, 1830, Blair-Lee Papers. Cf. the anti-Bank editorial printed in the *Washington United States Telegraph*, Dec. 16, 1829, with the Bank veto message, paragraphs 4, 5, 17, 47, *Messages and Papers*, ed. Richardson, II, 576, 577, 581, 591. Cf. also *Washington Globe* editorials of Jan. 19 and Apr. 27, 1831, with paragraph 36 of the veto message *Messages and Papers*, ed. Richardson, II, 586-87.

²⁰ "Speech of Mr. Clay."

²¹ In his *Autobiography* Van Buren was at pains to express his regret at having failed to gain access to what he called the "highest branches of learning." He had often felt his disadvantage "in my conflicts with able and better educated men." He repeatedly expressed this regret at lack of discipline in "mental habits." See *The Autobiography of Martin Van Buren*, ed. John C. Fitzpatrick, *Annual Report, American Historical Association*, 1918 (2 vols., Washington, D. C., 1920), II, 12.

with an office in the Treasury. A sallow, tubercular little fellow, painfully shy, he entirely lacked the figure and presence that could attract votes. He nevertheless had abilities. He possessed enormous industry, genius for practical administration, and a remarkable grasp of the principles of organizational efficiency. He could write, moreover, with clarity and effect. At every opportunity he counseled the Jacksonians: "Organize, organize."²²

How did this party organization differ from the American party previously most highly developed, the Republican party of Jefferson? That party had likewise possessed centralized control and extensive organization—"machinery" and "cadre" of a sort—as well as a coterie of party newspapers. But the national and state caucuses of elected representatives had dominated Jeffersonian party structure. Where the Jeffersonian party had been oriented primarily to established leaders, and only through and by them to popular support, the Jacksonian party went directly to the electorate. The faceless functionaries of the Jackson organization, from socially marginal "Kitchen Cabinet" members like Kendall or *Globe* editor Francis Preston Blair, down to lowly local postmasters and census takers laboring for the party, contrast sharply both with Jeffersonian behind-the-scenes organizers like John Beckley and with party mainstays like Aaron Burr, James Madison, and Alexander Dallas.²³

²² Kendall to Blair, May 10, 1829, Blair-Lee Papers. Among many other productions, Kendall was responsible for the veto message almost in its entirety, although several others did editorial work on it. Historical scholars have subsequently become dreadfully confused about the veto's authorship, but it was known in Washington, well before the message appeared, precisely who was composing it. For a detailed examination of the authorship of the message, see Lynn Marshall, "The Authorship of Jackson's Bank Veto Message," *Mississippi Valley Historical Review*, L (Dec. 1963), 466-77. Research subsequent to the publication of this article, incidentally, has turned up abundant corroborating evidence of Kendall's authorship. For evidence of Kendall's social status, see Alfred Balch to Nicholas Trist [Sept. 1831], Nicholas Trist Papers, Manuscript Division, Library of Congress. This genteel observer and loyal Jacksonian, having seen Kendall at a presidential dinner, thought his "watchfulness and awkwardness" contrasted poorly with "the polished conversation the graceful manner & high tone" of the real elite. "To me he does not look like a Gentleman and therefore I could not talk to him. . . ."

²³ An example of the Jackson party's efficient functionaries was a lowly groceryman in Frankfort, Kentucky. He served also as a very efficient postmaster. (Blair to Van Buren, July 10, 1838, Martin Van Buren Papers, Manuscript Division, Library of Congress.) Among several recently published, provocative studies of party organization in the period 1790-1815, those of Noble Cunningham, Jr., are perhaps the most suggestive: *The Jeffersonian Republicans: The Formation of Party Organization, 1789-1801* (Chapel Hill, N. C., 1957), and *The Jeffersonians in Power: Party Operations, 1801-1809* (Chapel Hill, N. C., 1963). See also William N. Chambers, *Political Parties in a New Nation: The American Experience, 1776-1809* (New York, 1963); Paul Goodman, *The Democratic-Republicans of Massachusetts: Politics in a Young Republic* (Cambridge, Mass., 1964); David H. Fischer, *The Revolution of American Conservatism: The Federalist Party in the Era of Jeffersonian Democracy* (New York, 1965). For the preparation of this paper, furthermore, James Banner made available his thought-provoking unpublished article, "The Federalist Party Organization in Massachusetts, 1800-1815." These studies have pioneered a significant new perspective for viewing American politics. It is the thesis of the present paper that this eminently useful perspective should now be broadened in scope, so as to view party organization in its further development through

Beckley, in contrast to the Jacksonian organizers, was a particular protégé and confidant of Virginia's leading gentlemen-politicians of Republican persuasion, Jefferson, Madison, and James Monroe. A polished alumnus of William and Mary, Beckley had been co-opted into politics originally by the local gentry of Henrico County, Virginia, at the age of seventeen and entered national politics by a similar process after his Virginia gentlemen friends had first made him clerk of the House of Burgesses and then clerk of the federal House of Representatives. National organization of the Republican party had begun in Congress, thereafter radiating out to local areas, and in this process Beckley was perfectly placed to play a leading role. The Republican party, first a faction of established leaders aligned around a common ideology, reached out through men like Beckley to other local gentlemen-leaders of similar persuasion. Beckley, Madison, and other Republican organizers, whether operating in Virginia, Pennsylvania, New York, or elsewhere, directed their attention primarily to what they variously described as "respectable persons," "prominent characters," "persons of influence," "suitable characters," or "gentlemen" with "influence among their neighbors."²⁴ The party operated "particularly by setting on foot expressions of the public mind in important counties, and under the auspices of respectable names."²⁵

Contrast this Jeffersonian style of organization with the spirit of the following "Kitchen Cabinet" proposal made in January 1830, and seriously entertained, although seemingly never implemented. The party must organize, it was proposed, in such a way as to circumvent the political dominance of the local gentry established as leading county lawyers, justices in the local courts, and sheriffs. The mass of Jackson voters, "plain farmers and me-

the Jacksonian era in the context of concurrent development in other large-scale organizations as well as in social structure generally.

²⁴ See, e.g., Dallas to Albert Gallatin, Jan. 16, 1805, in Henry Adams, *Life of Albert Gallatin* (Philadelphia, 1879), 327-28; Jefferson to Archibald Stuart, Feb. 13, 1799, *The Works of Thomas Jefferson*, ed. Paul L. Ford (12 vols., New York, 1904-1905), IX, 44; and other correspondence of Jefferson, Monroe, Madison, Burr, and Gallatin in this period. See also, for similar language, Beckley's correspondence with General William Irvine, a respectable gentleman-leader of the Republican party in central Pennsylvania, as extracted in Noble Cunningham, Jr., "John Beckley: An Early American Party Manager," *William and Mary Quarterly*, XIII (Jan. 1956), 40-52. About Beckley, Cunningham concludes: "It is of no little significance that Beckley's efforts were, to a large extent, directed toward winning the support of important citizens who could be counted upon to have influence on their neighbors." (Cunningham, *Jeffersonian Republicans*, 105.)

²⁵ Monroe to Madison, Oct. 9, 1792, *The Writings of James Monroe*, ed. Stanislaus M. Hamilton (7 vols., New York, 1898-1903), I, 243; Jefferson to Madison, Sept. 1, 1793, *Works of Jefferson*, ed. Ford, VIII, 14; Madison to John Taylor, Sept. 20, 1793, as quoted in Cunningham, *Jeffersonian Republicans*, 57; Madison to Jefferson, Aug. 27, Sept. 2, 1793, *The Writings of James Madison*, ed. Gaillard Hunt (9 vols., New York, 1900-10), VI, 179, 191-93; Madison to Archibald Stuart, Sept. 1, 1793, *ibid.*, 190; see also Cunningham, *Jeffersonian Republicans*, 57-60. Note, incidentally, that Jefferson used the name "Whig" for his new party at this time.

chanics," should therefore meet in simultaneous local conventions, too numerous for the local establishment to attend all at once, and pass resolutions suggesting candidates for office. "By these means," it was hoped, "the people would take the government into their own hands . . . and an active class of politicians would spring up opposed to them [the local establishment]."²⁶

Whence came the new Jacksonian mode of party organization? Sophisticated popularly based party structures, on a small scale, had developed in various states by the 1820's. Although scholarly studies of these parties have not, for the most part, treated organizational structure per se (preferring to emphasize ideology and colorful individual careers), it appears that several state parties, at least, developed in this period a Jacksonian type of impersonal structure by-passing established local elites. The most significant for the national organization of the Jackson party was the Kentucky Relief party, which coalesced out of general popular demand for relief laws for debtors following the panic of 1819. Here, men destined to become important in national Jackson party councils received apprenticeship in popular politics. *Globe* editor Blair and efficiency expert Kendall simply transferred Kentucky techniques of the 1820's directly to the national arena.²⁷ In New York, moreover, Antimasonic and Van Burenite organizations alike had developed centralized, specialized party apparatus. Antimasons built upon popular antipathy to a secret elitist group suspected of conspiring to obtain special privileges, just as the Jacksonians would in the Bank veto message.²⁸ The advent of Jackson as a presidential candidate, with his great appeal to ordinary voters throughout the nation, suggested the viability of political organization on this model at the national level.

The new approach to politics, however, was part of a general change in American society. It represented a new approach to social organization for the accomplishment of any sort of practical purpose. Jacksonian national party development related very closely to the Jackson administration's development of new organizational principles for the government's executive departments. These organizational principles, in turn, developed directly from practical administrative problems that the Jacksonians faced when they entered office, rather than from any preconceived ideology. When in the

²⁶ Kendall to Blair, Jan. 28, 1830, Blair-Lee Papers. This too represented a continuation of efforts first begun by Kendall and Blair in Kentucky politics during the 1820's.

²⁷ Lynn Marshall, "The Genesis of Grass-Roots Democracy in Kentucky," *Mid-America*, XLVII (Oct. 1965), 269-87; Arndt M. Stickles, *Critical Court Struggle in Kentucky* (Bloomington, Ind., n.d.).

²⁸ Jabez D. Hammond, *History of Political Parties in the State of New-York* (4th ed., 2 vols., Cooperstown, N.Y., 1846), II, 384-85; Lee Benson, *The Concept of Jacksonian Democracy: New York as a Test Case* (Princeton, N.J., 1961), 21-27. Although opportunistic Antimasonic leaders aligned New York Antimasonry against Jackson in 1832, this does not diminish the essentially "Jacksonian" character of the new mode of organization they represented.

Spring of 1829 the Jacksonians swept in, bent on "reform" as announced in Jackson's inaugural address, they set forth no new principles of organization. After a few months in office, however, they proclaimed in Jackson's first message to Congress their principles of "rotation in office."²⁹

It is significant that the Jacksonians did not propose merely to replace rascals in office with men of honor from their own party, as might well have been expected had they been interested in "spoils" only. They certainly rewarded friends and punished enemies about as much as they were able in the award of offices, following the custom in American politics dating from the inception of party contests. The opposition legitimately screamed "spoils," but the moralistic condemnations of the "spoils system" as something peculiarly Jacksonian, which have continued almost without pause ever since, have obscured profoundly important organizational innovations introduced by the Jacksonians. In so far as these innovations were concerned, the spoils question was entirely irrelevant.³⁰ The Jacksonians used the concept of rotation in office as a cloak for organizational changes that might otherwise have appeared revolutionary. By 1829 the idea of rotation in office was neither novel nor did it imply any social change, for it had been proposed on various occasions since the time of the American Revolution, and it was occasionally implemented. South Carolina, doubtless the most conservative and class-controlled state in the Union, provided the leading example of that doctrine in 1830.³¹

Under this cover the Jacksonians proposed to organize the executive department as a rationalized complex of offices, ordered by function, and defined by rules and regulations, so as to be free in so far as possible from irregular custom and individual personalities. In this system individuals could be placed or replaced without upsetting the integrity of the whole. Men were fitted to this system, not it to men.³² It was the administrative counterpart of

²⁹ *Messages and Papers*, ed. Richardson, II, 438-49.

³⁰ Historians seem to have been so mesmerized by the moralistic fervor of the civil service reformers of the late nineteenth century that they continue tirelessly to repeat their righteous condemnations of the Jacksonian spoils system. Leonard D. White, in *The Jacksonians: A Study in Administrative History, 1829-1861* (New York, 1954), has pointed out that the Jacksonians initiated administrative reform. The present paper is not meant to imply that spoils are efficient, but only that there are other more significant issues with which to deal.

³¹ Carl Russell Fish, *The Civil Service and the Patronage* (New York, 1905), 79-104. Jefferson himself had proposed that rotation be written into the Constitution of 1787. (Jefferson to Madison, Dec. 20, 1787, *Works of Jefferson*, ed. Ford, V, 372.)

³² This is what the great historian and social analyst Max Weber has called "bureaucratization." See "Bureaucracy" in *From Max Weber: Essays in Sociology*, ed. H. H. Gerth and C. Wright Mills (New York, 1946), 196-244. American "bureaucracy" differed in very important respects from Weber's ideal, however, for Weber emphasized professionalization and life tenure in officeholders. He admired "the apparatus" of administration itself and placed great value on maintaining its integrity. The American variety of "bureaucratic" social organization, as hereafter sketched, essentially the organizational model for the modern industrial world, has defined itself rather differently.

the interchangeability of machine parts. Jackson's rotation proposal explicitly denied that office should become "a species of property" or be used to support "the few at the expense of the many." "The duties of all public officers," the message announced, "are, or at least are capable of being made, so plain and simple that men of intelligence may readily qualify themselves for their performance. . . ." In spite of much emphasis on equality of opportunity for all citizens, the Jacksonians justified this system mainly by the "efficiency of Government" that it might bring.³³ The signal organizational insight was this: efficiency lay primarily in the system (rules and regulations) rather than in men (character).

The Jacksonian proposal of rotation in office was never actually put into practice, and the administrative principles it masked were only falteringly applied to the organization of government departments. The post office reorganization of 1836 was its most important product.³⁴ In part this resulted from opposition to the new system in Congress, which had to provide the basic rules and regulations of office. In part it resulted from opposition within the Jackson party itself, which, after all, was still in a process of development. The new administrative system eventually had its greatest effect outside the realm of government altogether: in large-scale private business. In its "rotation" form, furthermore, the innovative principle of the system—the interchangeability of human parts—was overstated. In any absolute sense it could not work and was not so intended by its authors. Certain offices required expert skills. That did not make them, however, any the less definable by impersonal rules or less subject to later functional redefinition.

It was not that American government had never previously been organized according to rational and functional principles. The government departments had been originally set up on a plan derived from a venerable tradition of bureaucratic structure.³⁵ Alexander Hamilton, for example, had consciously modeled the new Treasury Department upon its English counterpart. The British Exchequer itself belonged to one of the rationalized bureaucracies long since established during the rise of monarchical national states in Europe. These bureaucracies, however, had established and maintained prestige largely by filling their ranks with careful attention to pre-existing social gradations. Thus it had been with Hamilton's system too. The government would gain in respectability, President Washington had agreed, in proportion to the respectability of the officers serving it. Hamilton had

³³ *Messages and Papers*, ed. Richardson, II, 449.

³⁴ 5 US Statutes at Large 80 (July 2, 1836).

³⁵ Ernest Barker, *The Development of Public Services in Western Europe, 1660-1930* (London, 1944), 1-12; Hans Rosenberg, *Bureaucracy, Aristocracy, and Autocracy: The Prussian Experience, 1660-1815* (Cambridge, Mass., 1958), 1-25, 227-28.

therefore chosen men of character and "standing in the community" to fulfill Washington's intent that officials "give dignity and lustre" to their offices.³⁶

The application of such social criteria to officeholders rigidified the system and inhibited its efficiency. It was thought that a man once placed in office ought not properly to be removed, except for the grossest misconduct, nor the office changed. The character of the officeholder, once appointed, largely defined the office; his removal or any arbitrary change in his office reflected on his character and was damaging to the whole social system. Consider the case of Federalist Timothy Pickering's clerks in the State Department whose removal, after they had been discovered levying strictly illegal fees from applicants for passports, Pickering greatly regretted because they had "sustained fair characters" in the community at large.³⁷ Jefferson had acted upon similar premises. Government administration was to him a matter primarily of "virtue and talents." "The whole art of government," he said, "consists in the art of being honest."³⁸ The Jacksonian alternative, masked in the proposal of rotation in office, offered to increase efficiency by ignoring pre-existing social criteria like "character" and "respectability" and defining office impersonally, entirely by rules and regulations.

The Jackson press explicated and justified in various ways the new system announced in the rotation proposal. Throughout 1830 and 1831, and almost down to the moment of the Bank veto, "reform" provided their main issue of party agitation. In paper after paper the Jackson organs ran elaborate computations of how much money their administrative reforms had saved the government.³⁹ These and a multitude of other articles on "reform" made no distinction between general improvements in the efficiency of the system and the money saved by eliminating corruption in officeholders. Any sort of efficiency, as measured by money saved, was equally moral, and any sort of inefficiency, corrupt or otherwise, was equally immoral. While the Jacksonians were exceedingly moralistic and personal in their condemnations of the old administration, they championed a new system to replace it that was remarkably impersonal and strictly regulation. This was to be the officeholder's creed: "I want no discretion. I wish to be able to turn to some

³⁶ Leonard D. White, *The Federalists: A Study in Administrative History* (New York, 1948), 118, 126, 257-58, and *The Jeffersonians: A Study in Administrative History, 1801-1829* (New York, 1951), 347-68.

³⁷ *Id.*, *Federalists*, 286-87.

³⁸ See Jefferson's "A Summary View of the Rights of British America," Aug. 1774, *Works of Jefferson*, ed. Ford, II, 88.

³⁹ See, e.g., "The Black List," *Washington United States Telegraph*, Aug. 14-Sept. 24, 1830. The *Washington Globe* reprinted the same series, in corrected and amended form, in June of the following year.

law or lawful regulation for every allowance I am called upon to make."⁴⁰

The Jackson press had devoted a considerable proportion of its columns to justifying removals from office. When the opposition shouted "spoils," the Jackson press promptly showed, with remarkable specificity, how the officeholder's books had been in arrears and his accounts carelessly and irregularly drawn, concluding with an exact calculation of the sum lost to the government thereby. The opposition customarily replied with an article ostensibly written by the officeholder himself, but really by some such proto-Whig leader as William Wirt, who condemned the charges as "egregious insolence" and "a pitiful attempt to dishonor the word of a gentleman" by citing "paltry little omissions" (in one case, "that paltry little sum of \$3835"). To this the Jacksonians responded in simple, direct, and telling style. Let the regulations of office be strictly obeyed, they said. Books should reflect reality, not false or misleading entries based on customs and justifiable only by appeal to the officeholder's honorable position in the social system. Regulations, not the social status of the officeholder, defined the office. "Property in office," they said, provided the basis for "aristocratic" control of government.⁴¹

The proto-Whig tacticians faced in an entirely different direction. In the masterful plan of operations they worked out for the campaign of 1832 they sought to gather all respectable political leaders under the "executive usurpation" banner, with the tacit assumption that they would bring with them their local constituencies—precisely the system theretofore traditional in American politics. They forced the recharter bill through, obtained the President's veto, driving thereby a wedge between Jackson and many of his original party leaders, and skillfully developed the Webster-Clay-Calhoun alignment around the "executive usurpation" position, but they failed utterly to take their principal objective, the presidential election. These battle-proven leaders seemed so rigidly to follow their plan of operations, drawn up according to well-established principles of political warfare, that they failed to comprehend the irresistible power of the new weapon introduced by the Jacksonians. Or perhaps they understood too well, but remained so attached to traditional political modes that they could not give them up without first doing everything in their power to defend them. It is altogether remarkable

⁴⁰ Fourth Auditor's Report, Nov. 30, 1829, *Washington United States Telegraph*, Dec. 10, 1829.

⁴¹ See, e.g., the disputes over Miles King, the removed naval agent at Norfolk, Virginia, and Major William Barney, removed "Superintendent of Light Houses and Buoys." (*Washington United States Telegraph*, July 5, 8, 20, Aug. 24, Sept. 21, 1830; Mary Barney to Jackson, June 13, 1829, *Correspondence of Andrew Jackson*, ed. John Spencer Bassett [7 vols., Washington, D. C., 1926-35], IV, 48; Mary Lane to Mary Barney, Aug. 9, 1830, reprinted from the *Laurenceburg Indiana Palladium* in the *Washington United States Telegraph*, Sept. 21, 1830.)

how many respectable political leaders were split away from the Jackson party without apparently impairing its effectiveness at the polls: Samuel Ingham, John M. Berrien, John Branch, John C. Calhoun, John McLean, Louis McLane, Littleton Tazewell, John Tyler, John Eaton, Hugh L. White, to name a few. These men shared common characteristics—stuffiness, social pretension, and great concern for honor, dignity, and decorum. Thus, curiously, from the outset the proto-Whigs methodically maneuvered as if bent on self-destruction, reinforcing their own anachronism by publicly eschewing direct popular appeal and gathering into their ranks as many established leaders as possible, while forcing the Jackson party to increase its voter orientation.

This party distinction was not absolute, however, for at least some proto-Whigs were not above trying to organize in the Jacksonian style or applying what was condescendingly called “*ad captandum*” (pleasing to the crowd) techniques. In their own way they had always wooed the electorate anyway. Clay himself, for example, received the plaudits of several of his party cohorts for frankly “*ad captandum*” antiforeign remarks in his widely distributed tariff speech.⁴² In his vicious and personal attack on the venerable free trader, Albert Gallatin, whose half century in America could not make him more to Clay than “still at heart an alien,” Clay hissed, “Go home to your native Europe.”⁴³

The proto-Whig alignment, moreover, included the Antimasonic party, a relatively small but highly organized and distinctly voter-oriented party. Some of these proto-Whigs understood the sources of Jacksonian party strength well enough to see clearly the power of the veto message, refusing sometimes even to publicize Webster’s speech for fear it would further alienate the electorate. Clay understood the import of the veto at least enough to stop Nicholas Biddle from printing and distributing it generally as a pro-Bank handbill, for that benighted representative of genteel Philadelphia erudition had thought the message so obviously inappropriate and reprehensible as to make it Bank propaganda. In the midst of the election campaign, furthermore, when proto-Whigs in Virginia seemed ready to give up in despair, Clay counseled fighting fire with fire. “Let our friends organize . . .,” he said, proposing a “central committee” with local committees to “bring the voters to the polls.”⁴⁴ Even this proposed elaboration of organization, however, retained the flavor of the older model of party structure, an organization of respectable “friends” who condescended to “bring” in the votes.

⁴² James Barbour to Clay, Mar. 7, 1832, *Works of Clay*, ed. Colton, IV, 328.

⁴³ Adams, *Gallatin*, 641–42.

⁴⁴ Clay to Biddle, Aug. 27, 1832, Nicholas Biddle Papers, Manuscript Division, Library of Congress; Clay to Francis Brooke, Aug. 5, 1832, *Works of Clay*, ed. Colton, IV, 341.

The Jackson party, on the other hand, offered no perfect voter orientation either. At Jackson's first inauguration in 1829, that party had offered an alignment of ideologically diverse leaders very similar to the proto-Whigs of 1832. The party arrived at its voter orientation, as expressed in the veto message, haltingly and stumblingly. The 1831 cabinet crisis, revolving around Peggy Eaton, the tavernkeeper's daughter who married the Secretary of War, culminated in an intraparty rash of threatened duels. Such preoccupation with honor and decorum was characteristic of a highly traditional role of leadership. The new 1831 cabinet followed the traditional model of political leadership once more, attempting to weld together a group of socially respectable leaders like Edward Livingston and Louis McLane. Unfortunately the sympathies of these respectables turned out, many times, to lie with the proto-Whigs. McLane, Secretary of the Treasury, offered the prime example of this, becoming a most annoying irritation on the Bank issue. The Jacksonians' attempt to play down that issue in the President's message of December 1831 likewise represented an essentially leader-oriented gambit. They had to be pushed by the opposition, finally, into the Bank issue and frank voter orientation.

Inconsistencies within both parties suggest that each was moving generally in the same direction, in transition from leader orientation to voter orientation, although the Jackson party had advanced further. The Bank issue, however, was perfectly calculated to accelerate the development of the Jackson party and decelerate the opposition, pushing the parties apart until they offered a striking contrast in orientation. The Whigs attempted to exploit their leader orientation to the fullest even after defeat in the election of 1832, achieving their greatest success in 1834 with the Senate censure of Jackson. Their failure in 1832 resulted, they supposed, from Jackson's great popularity as a military hero, not his party organization and electioneering techniques. The decisive defeat in 1836 by less than heroic Van Buren, however, proved without doubt the utility of Jacksonian party organization and techniques. By 1840 the Whigs had adopted these techniques. Even the eminent Webster would brag to voters about his father's log cabin, stumping for such Whig candidates as "Old Tippecanoe," pseudo hero William Henry Harrison.

The two party types represented two distinctly different views of society at large. When the Whigs agitated the question of "spoils," for example, they dealt not so much with technical government administration as with general social values. Government service, in their view, ought to be a function of private social status; it had traditionally been so considered. They looked back to a world in which a gentleman, once appointed to an office, private or

public, expected to be continued therein, and the functions of his position fitted to his capabilities. Though not quite "property in office," as the Jacksonians called it, this view emphasized long tenure based on criteria non-functional to the operations of the organizations involved, be they government bureaus, private businesses, or even churches. The Whigs were not terribly concerned about "spoils" proper; they objected not to the reward of "friends," but to the social character of the friends rewarded.

The Whiggish view looked back to a society embodying the Lockean liberalism of the eighteenth century. In it, all affairs, political or otherwise, moved under the effective control of sagacious men, each within his own locality sufficiently pre-eminent economically, intellectually, and socially to transcend immediate popular control even if the franchise were widely distributed. Greatest emphasis was placed upon the liberty of the individual to express himself, if he were able and sufficiently educated, in great social theories and high ideals. American constitutions embodied such theories. Hamilton's plan for organization of the Treasury Department likewise exemplified a part of a liberal theory of this sort, as did Gallatin's 1808 scheme for a system of internal improvements, Jefferson's education scheme for Virginia, and the handsome proposals offered by John Quincy Adams in his 1825 inaugural address. Liberal partisans of Republican and Federalist political alignments entertained important ideological differences, but they shared basic assumptions about social structure.

The Jeffersonians envisioned a locally established intellectual elite handing down great humane theories from on high, a conception that differed from the Hamiltonian only in the assumption that the theories would, if worthy, necessarily receive ratification from an enlightened populace. Political parties were defined as like-thinking alignments of sociopolitical leaders; it was always hoped that leaders would reach, through rational discussion, such a consensus as to eliminate the need for parties of any sort. It was quite possible to be both nationalist in ideology, like the Federalists, and to derive social status and the claim to political leadership primarily from membership in a local social establishment.⁴⁵

⁴⁵ Cf. Sidney Aronson, *Status and Kinship in the Higher Civil Service: Standards of Selection in the Administrations of John Adams, Thomas Jefferson, and Andrew Jackson* (Cambridge, Mass., 1964). This study attempts to compare the higher echelons in the administrations of John Adams, Jefferson, and Jackson, assigning to the Jeffersonians the main responsibility for democratic change, while denying the Jacksonians anything more than a minor and subsidiary role. Although commendable for attempting rigorous quantitative analysis of this distant and difficult area, its results are rendered inconclusive by substantial technical shortcomings. To cite one of several similar weaknesses, the study compares the social status of officeholders in the Jackson administration with those of the Jefferson administration, which preceded it by twenty years, as if this were no different from comparing the Jefferson administration with the Adams administration immediately preceding it. See also another more solid quantitative study, Richard P. McCormick, "New Perspectives on Jacksonian Politics," *American Historical*

The Constitution of 1787 perfectly illustrated the proper role of political leadership according to the conception of the eighteenth-century liberal. The Constitution was par excellence a noble theory of government, composed in secret by a group of eminently respectable political leaders representing various local establishments, and handed down to the electorate for ratification. If a majority of the delegates in Philadelphia turned out eventually to be Federalists, a proportion of them became Republicans, including, of course, the most prominent of them all, Jefferson's hand-picked successor to the presidency, James Madison. Jefferson himself quite approved the process by which the Constitution was drawn up. If he entertained misgivings about some of its provisions, at least until the Bill of Rights was added, he fully approved the leadership role it embodied.⁴⁶ That leadership role, mutually respected by Federalists and Republicans, the proto-Whigs sought only to continue.⁴⁷

The proto-Whigs sought to emulate Hamilton and Jefferson and to produce great liberal theories. Clay's "American system" was but a pale reflection, perhaps, but it was nevertheless an effort in this direction. Calhoun's "con-

Review, LXV (Jan. 1960), 288-301. McCormick, however, has concluded from a careful examination of the numbers of voters participating in elections that the advent of Jackson involved no "mighty democratic uprising." By arbitrarily defining democracy simply as proportion of possible voters participating, however, McCormick ignored the effective power of the electorate as influenced by party structure. See also *id.*, *The Second American Party System: Party Formation in the Jacksonian Era* (Chapel Hill, N. C., 1966), for a view of party formation contrasting with that of the present paper.

⁴⁶ Jefferson to Madison, Dec. 20, 1787, *Works of Jefferson*, ed. Ford, V, 370-75. It is suggestive that Jefferson here expressed some doubt about the scope of power given the House of Representatives, the most democratically elected body in the proposed government. He approved the executive veto explicitly, moreover, and even suggested that a judicial veto be included as well. Jefferson and Adams, by settling down in their dotage to a long, warm correspondence, showed how akin they really were. "For I agree with you," wrote Jefferson, "that there is a natural aristocracy among men. The grounds of this are virtue and talents . . ." (Jefferson to Adams, Oct. 28, 1813, *The Adams-Jefferson Letters: The Complete Correspondence between Thomas Jefferson and Abigail and John Adams*, ed. Lester J. Cappon [2 vols., Chapel Hill, N. C., 1959], II, 387-92.)

⁴⁷ Cf. Chambers, *Political Parties*, 95-109. Chambers has identified a significant change from an elitist, "plebiscitarian," "party of notables" Federalist party, to a truly democratic Republican party, a "party of politicians." He has derived this conceptual framework from Max Weber's essay "Politics as a Vocation." (See *From Max Weber*, ed. Gerth and Mills, 77-128.) These typologies resemble the distinction drawn in the present paper between leader orientation and voter orientation, but Chambers has followed Weber's model too closely and has the full scope of the development too early. Chambers has also suggested the importance of party organization to Jacksonian political developments, in "Party Development and Party Action: The American Origins," *History and Theory*, III (No. 1, 1963), 93, n. 6. Both this and his above-cited book cogently urge the utility of conceptual framework in the study of American party development. On Jacksonian political organization, see also Moisei Ostrogorski, *Democracy and the Organization of Political Parties* (2 vols., New York, 1902), II, 39-79. Weber, in "Politics as a Vocation," evidently based his analysis of American parties on Ostrogorski, and Weber too became preoccupied with righteousness about spoils. Note, however, Weber's identification of "bureaucracy" with "mass democracy" specifically in American political party development. "In the United States, both parties since Jackson's administration have developed bureaucratically." (*From Max Weber*, ed. Gerth and Mills, 224-25.)

current majorities,” if strained and self-serving, came closer to the mark. Clay’s “Ashland” and Calhoun’s “Fort Hill” aped “Monticello” and Mount Vernon in emphasizing local attachment to native state and gentry. The Jacksonian reorganization of politics threatened to destroy all this, and the proto-Whigs drew together in a last-ditch effort to defend it. That the two surviving members of the old Republican triumvirate, Madison and Gallatin, should align themselves automatically with the proto-Whigs in 1832 was hardly surprising. Gallatin had long distrusted what would come to be the Jacksonian party type. He had counseled Jefferson in 1801 that they ought greatly to fear “men whose political existence depends on . . . party.”⁴⁸

The proto-Whig party alignment ought properly to have been restricted along ideological lines either to like-thinking nationalists, or, alternatively, to states’ righters. Under the dire threat offered by the Jacksonians, however, the party included all who stood for the traditional leadership role, whatever their ideology. Thus did Calhoun embrace Webster and Clay even while leading the nullification fight. In so doing they looked back longingly to a heroic era when leadership in politics was integral to leadership in society. They reached the *reductio ad absurdum* of their anachronistic localism when, in the 1836 campaign, they put several local presidential candidates into the field simultaneously, to be beaten en masse by Van Buren. Thus was the Whig party born dead in July 1832 and continued in that condition until 1836. Thereafter, however, a total transfusion of Jacksonian blood would miraculously bring it to life.

Alexis de Tocqueville sensed in the United States of 1831–1832 the already half-realized sociopolitical changes against which the proto-Whigs reacted. He saw in a possible “tyranny of the majority” the subversion of the traditional type of locally based, socially secure, political leadership with which he himself identified. Historians have much praised, condemned, or explicated Tocqueville’s thought on this point, but have failed to point out that his “tyranny of the majority” was in many respects exactly equivalent to proto-Whig “executive tyranny,” including its primary leader orientation. At the very crux of his discussion of the power exercised by the majority over contrary opinions, he noted that, although an individual in the United States may be allowed perfect freedom of expression within certain limits, he absolutely dare not go beyond them. Not that his life would be endangered, but “his political career is closed forever.” “You may retain your civil rights,” said he, “but they will be useless to you, for you will never be chosen by your

⁴⁸ Gallatin to Jefferson, Aug. 10, 1801, Adams, *Gallatin*, 278.

fellow citizens if you solicit their votes. . . ." In this way the individual would be "deprived of the rights of mankind."⁴⁹

The Bank of the United States embodied just this leadership ideal championed by Tocqueville and the proto-Whigs. Without denying the obvious economic utility of central control on banking, consider the socially impacted structure of this particular institution. Originally constructed in accordance with a segment of Hamilton's brilliant theory, it represented a grand scheme with which men of honor might reach out imaginatively to secure possibly great benefits for the whole of society. It represented, pre-eminently, government buttressing of private socioeconomic position. Its enormous economic power was only nominally limited by public opinion, and the only real limitation lay in the honor and intelligence of its essentially dilettante-banker executives. The Bank's structure conformed closely to local social establishments. Each of its many branches had its own board of directors drawn from the local gentry, planters, and merchants, who were generally politicians too, and dilettante bankers all. Only the cashier in each branch represented directly the central institution in a full-time capacity. Obvious social criteria were used in the selection of cashiers, just as they had been used in the selection of Biddle himself as president of the mother bank. For all the banking skill he acquired subsequently, Biddle had been invited into the Bank not on the basis of experience as a banker, in which he was woefully weak, but on his general character, his "virtue and intelligence."⁵⁰ The Bank issue in 1832, therefore, was perfectly calculated to point up fundamental social changes then in process.

If the Whiggish ideal seems strange and remote in many respects, the ideal implied in Jacksonian innovations seems familiar by contrast. If the one harked back to the past, the other offered a vision of the future—although usually represented by the Jacksonians themselves as an ideal deriving from a more perfect era in the American past. The Jackson party forever talked of states' rights and paraded Jeffersonian rhetoric, straining to appear traditional. Actually, of course, the Whigs more resembled the Jeffersonians than did the Jacksonians. The Jacksonian ideal, while couched in Jeffersonian shibboleths, involved no great concern for local protection of individual liberty.

Since at least the close of the War of 1812 Americans had been experimenting enthusiastically with new modes of social organization, for pur-

⁴⁹ Alexis de Tocqueville, *Democracy in America* (2 vols., New York, 1945), I, 274-75. For development of related elements in Tocqueville's work, see Seymour Drescher, *Tocqueville and England* (Cambridge, Mass., 1964), 74-104, and "Tocqueville's Two Democracies," *Journal of the History of Ideas*, XXV (Apr.-June 1964), 211-16.

⁵⁰ Biddle to John McLean, Jan. 10, 11, 1829, Biddle Papers; Thomas P. Govan, *Nicholas Biddle* (Chicago, 1959), 83-84.

poses spiritual, practical, or merely for amusement. These new alternatives rapidly eroded away the old social order, once so firmly established in New England's townships and Virginia's counties. The new modes of social organization developed directly out of recognition of specific problems and attempts to solve them, as efficiently as possible, in keeping with certain assumptions about the nature of man. It had become obvious to Americans by that date that many things could be done to increase both material and spiritual comforts, things that could never be done by individual efforts of independent yeomen farmers of the Jeffersonian image however dutifully they followed the wise counsels of the rightful *aristoi*. Goods needed to be grown, manufactured, and shipped, messages sent, taxes collected, and souls saved on a grand scale. The situation seemed to demand social reorganization. The resulting American penchant for organization and reorganization, by all classes and for all purposes, startled a sensitive observer like Tocqueville. This period's efflorescence of different types of organizations has been much noted, but here again historians have not concerned themselves with organizational structure so much as with formal ideologies, just as in the case of political parties.

This era's enthusiastic activity in economic affairs—in finance, manufacturing, transportation, and merchandising—involved basic experimentation in modes of social organization as well as advance in economic productivity. These experiments ranged from the Boston Associates' highly successful work force of respectable unmarried farm girls, to Kentucky's ill-fated Lexington Manufacturing Company with its great imported steam engine and cadre of Yankee production "engineers." They ranged from the Erie Canal, to the West's myriad state-chartered banks, to New York City's far-flung new merchandising houses. Apparently not until the 1850's, however, did any really large-scale business organizations develop in the United States, excepting Biddle's Bank and John Jacob Astor's American Fur Company, both of which operated on an earlier organizational model. In the boom during the mid-1830's there were textile factories in the Philadelphia area employing more than two hundred operatives. Simultaneously, factory workers first organized themselves into large-scale labor unions. That the National Trades' Union crumbled in the panic of 1837, along with many burgeoning factories, detracts nothing from the social significance of such organizational attempts.⁵¹

⁵¹ Alfred D. Chandler, *Strategy and Structure: Chapters in the History of Industrial Enterprise* (Cambridge, Mass., 1962), 19–22. For the Lexington Manufacturing Company, see Ebenezer Stedman's charming recollections, published under the title *Bluegrass Craftsman: Being the Reminiscences of Ebenezer Hiram Stedman, Papermaker, 1808–1885*, ed. Frances L. S. Dugan and Jacqueline P. Bull (Lexington, Ky., 1959), 14–25; see also William A. Sullivan, *The Industrial Worker in Pennsylvania, 1800–1840* (Harrisburg, Pa., 1955), 17–23, 99–118.

The most profound experiments in novel modes of social organization involved affairs other than economic. It seems more than fortuitous, however, that important New York merchants like the Tappan brothers led in establishing a series of new religious-associated organizations for special purposes, broadly Protestant evangelical or moral. Many such groups sprouted in the 1820's. Organized by laymen, each extended over the entire nation. Interdenominational, they were in structure entirely outside either local churches or denominational organizations. The American Sunday School Union, the American Society for Promoting Temperance, the American Home Missionary Society, and several other groups quickly established themselves and grew at a rapid rate. Each soon claimed functionaries numbering in the hundreds, or even thousands, with annual budgets approaching \$100,000.⁵² Antislavery organization in the 1830's was but one more such effort.

In addition to such specialized organizations, there was an extraordinary variety of attempts at total social reorganization: the familiar communitarian experiments like New Harmony, Brook Farm, and the Mormon communities. These ranged in political structure from Mormon authoritarianism to transcendentalist freedom, from Shakerite celibacy in family relations to Oneidan free love, and, in modes of economic productivity, from foolish bucolic idyl to highly profitable manufacturing enterprise. The widespread expectation, furthermore, among various American Protestant groups during these years that the millennium was at hand seems related to recognition of the passing of accustomed social roles and social order. Jacksonian party development and administrative reform were just one more such experiment in social organization, but they were essentially the system that would ultimately triumph over all others.⁵³

The Jacksonians discovered the key to efficiency in an egalitarian ethic, that national principle which Americans generally had come to take for granted in the half century since the Declaration of Independence. The overwhelming triumph of the political party of Jefferson had helped to establish it, and the newness of all social institutions in America had doubtless buttressed it at least as much as formal liberal ideology. This egalitarian ethic provided a necessary foundation for Jacksonian changes in social organization. That the Jacksonians connected their "rotation in office" doctrine with egalitarianism was not simply opportunistic. Their recognition that ad-

⁵² William Warren Sweet, *Religion in the Development of American Culture, 1800-1840* (New York, 1952), 188, 262-71.

⁵³ For an account of one of the most organizationally experimental communitarian groups, see Fawn M. Brodie, *No Man Knows My History: The Life of Joseph Smith* (New York, 1945).

ministrative efficiency might be increased by establishing interchangeability of human "parts" required an egalitarian ethic; their faceless, specialized party organization did as well.

By starting with an assumption that, for organizational purposes, individuals could be considered as essentially equivalent, it then became possible to define offices by functional regulations only rather than by the personality or social status of the individuals who occupied them. An organization on this model gained efficiency by its flexibility. It could shift or replace personnel without impairing the integrity of the system as a whole. Offices themselves could be easily redefined so as to perform ever more efficiently the particular organizational function. A national political party should thus efficiently organize to manufacture public support for the administration or channel votes to it. Its principles of organization differed not at all from groups organized to administer the government or to perform economic and other private functions.

A year prior to the Bank veto message, the *Globe* explicitly described this vision. "Government is a *business*. It should be managed by *men of business*." Furthermore, "It is not for *show*; but for *use*." The function of government in society should not be "to make a few men *great*." Therefore, political leaders or officeholders "should not be raised by distinctive marks or unusual incomes, above their fellow-citizens"; nor should they have any "extraordinary dignity . . . attach to their stations." Government ought not to be considered the most important segment of society. In the ideal society, concluded the *Globe*, "In all that belongs to pomp and parade the rich citizen would excell the highest officer of government." Such a government would strictly tend to its business, furthermore, and not use its power to buttress social or economic establishments by granting "monopolies or exclusive privileges," an obvious reference to the Bank of the United States. "Under governments . . . administered by plain, industrious men who would as soon follow any other honest business, how happy would man be!"⁵⁴ The Bank veto message alluded to the same vision when it pronounced, with the same emphasis, "Banking . . . is a *business*. . . ."⁵⁵ While the proto-Whigs looked back to the role of political leaders like Washington, Hamilton, and Jefferson, this Jacksonian vision anticipated such opportunistic, popularly oriented

⁵⁴ Washington *Globe*, July 14, 16, 19, 27, 1831. Cf. Weber: "The idea that the bureau activities of the state are intrinsically different in character from the management of private economic offices is a continental European notion and, by way of contrast, is totally foreign to the American way." (*From Max Weber*, ed. Gerth and Mills, 198.)

⁵⁵ *Messages and Papers*, ed. Richardson, II, 587. Jefferson had also used the terms "system" and "men of business" in connection with party organization, but clearly meant something quite different by them. (See Jefferson to Caesar A. Rodney, Dec. 31, 1802, as quoted in Cunningham, *Jeffersonians in Power*, 75-76.)

politicians as Abraham Lincoln as well as such plain business organizers as John D. Rockefeller and Henry Ford. Kendall, architect of Jacksonian administrative reform, performed the same function, following his retirement from politics, for the early telegraph industry. The Jacksonian ideal, in short, envisioned a society made up of just the sorts of flexible, pragmatic organizations that have since become perhaps the most typical products of American culture.

As a political leader Jackson was a transitional figure. No matter how he might try to emulate Washington, the polished military paragon, or Jefferson, the humane political sage, he could not succeed. Jackson developed no elevated rational theory; he reacted directly to events, trusting to his intuitive grasp of immediate practical expedients. Thus he recognized the utility of efficient organization when he saw some particular practical function to be performed. Jackson's character may perhaps best be measured by his choice of chief political lieutenants. In Van Buren and Kendall he demonstrated his taste for the practical, the shrewd, the opportunistic, and the efficient—even though socially obscure.

No change occurred instantaneously in July 1832. The social movement in this period had begun long before and would continue for several decades, but the Age of Jackson, and specifically the veto of the Bank of the United States, was pivotal. Around this point a colonial order made itself over into modern industrial America. Political parties and government departments, especially the post office, were then the largest organizations in the country; the Jacksonian reorganization of them gave the first practical test to innovative techniques of large-scale rational organization on a peculiarly American model. American society would, before long, reorganize itself generally into a series of these great, specialized, flexible, rationally ordered systems, made up of mobile interchangeable operatives. The efficiency and productivity of those devoted to economic affairs would become the envy of the world. There has perhaps been no more sweeping and fundamental change in all of American history. The Whig party in the 1830's recognized it for what it was and fought valiantly against it, until the utter hopelessness of the struggle became too apparent.

Noncapitalist Wealth and the Origins of the French Revolution

GEORGE V. TAYLOR*

TO call the French Revolution of 1789 a "bourgeois revolution" invokes ideas which, by common consent, are inseparable from that phrase. It implies, for example, a social class created and nurtured by capitalism, with its wealth preponderantly capitalist in form and function and its values largely derived from capitalism. It implies that the relation of this class to the processes of production differed substantially from that of other classes and that, allowing for a reasonable number of eccentricities, the bourgeoisie showed an over-all unity of goals and outlook, related significantly to capitalism, that made its political action meaningful, powerful, and revolutionary. Stripped of these associations, the phrase "bourgeois revolution" (or "revolutionary bourgeoisie") loses most of its interpretive value, including particularly its involvement with a concept of economic change and class struggle ranging from the Middle Ages to the cold war and beyond.

The ideas that comprise this interpretation have now come under criticism, chiefly from Alfred Cobban. In his London inaugural lecture of 1954, in an article of 1957 on "The Vocabulary of Social History," and in his *Social Interpretation of the French Revolution*,¹ Cobban argues that the concepts embodied in the words "bourgeois revolution" disagree with what research has brought to light. He believes that the phrase incorporates a self-confirming system of deception. Taken in its ordinary sense, it acts as a standard for selecting, interpreting, and arranging evidence, and because of this the research usually ends by confirming assumptions that creep in with the terminology.² In the writings of Albert Soboul and the late Georges Lefebvre, Cobban finds assertions and data that can be turned against their conclusions, and he attributes these discrepancies to unperceived conflicts between their premises and their evidence. As a corrective, he calls for a reform of the

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¹ Alfred Cobban, *The Myth of the French Revolution* (London, 1955), "The Vocabulary of Social History," *Political Science Quarterly*, LXXI (Mar. 1956), 1-17, and *The Social Interpretation of the French Revolution* (Cambridge, Eng., 1964); see also his *Historians and the Causes of the French Revolution* (rev. ed., London, 1958).

² *Id.*, *Social Interpretation*, 13, 22.

vocabulary, challenging, among other things, the equivalence of "bourgeois" to "capitalist," and of "noble" to "feudal," and there are others who share his dissatisfaction. I have myself found that there were under the old regime not one kind of capitalism but three, that in comparison with nineteenth-century capitalism they were relatively primitive, and that nobles held a heavy stake in two of them.³ It also seems clear that the speculation and stockjobbing of the 1780's at Paris, so conspicuously capitalist in appearance, was built not on the modernization of industry and trade but on the financial needs and policies of the monarchy. It was heavily penetrated by the nobles, and its center of gravity included the royal court as well as the Bourse.⁴ Herbert Lüthy complains that a "quasi-Marxist" preoccupation with capitalism as peculiarly bourgeois has obscured the capitalism of the court and the great nobles and diverted research from the study of the fortunes of the "grandees."⁵ Finally, in a recent article, Elizabeth Eisenstein shows that Lefebvre, in his *Coming of the French Revolution*, attributed the initial stimulus of the "bourgeois revolution" to a group of nobles, the Committee of Thirty, apparently without noticing the contradiction between their status and the class character of the revolution they were supposed to have set afoot.⁶

All this suggests that what has long seemed a settled explanation of the French Revolution has become the source of growing dissatisfaction and is up for a reappraisal like that which J. H. Hexter has applied to the concepts of the gentry and the middle class in Tudor England.⁷ This reappraisal is far from complete. The range of topics involved in a full examination of the bourgeois revolution model is very broad; the issues are economic, social, political, and even intellectual. The problem can be taken up at several points. Cobban, in his *Social Interpretation*, reopens the question of how the bourgeois revolution was related (if at all) to that of the peasants, whether the Revolution strengthened capitalism or weakened it, and whether the real winners of the Revolution were not the landowners rather than the commercial-industrial entrepreneurs. Another issue is posed by recent studies of social structure that show wide ranges of property and income within

³ George V. Taylor, "Types of Capitalism in Eighteenth-Century France," *English Historical Review*, LXXIX (July 1964), 478-97.

⁴ *Id.*, "The Paris Bourse on the Eve of the Revolution, 1781-1789," *American Historical Review*, LXVII (July 1962), 951-77.

⁵ Herbert Lüthy, *La banque protestante en France de la Révocation de l'Édit de Nantes à la Révolution* (2 vols., Paris, 1959-61), II, 687.

⁶ Elizabeth L. Eisenstein, "Who Intervened in 1788? A Commentary on *The Coming of the French Revolution*," *American Historical Review*, LXXI (Oct. 1965), 77-103.

⁷ J. H. Hexter, "The Myth of the Middle Class in Tudor England" and "Storm over the Gentry," which are conveniently brought together in his *Reappraisals in History: New Views on History and Society in Early Modern Europe* (New York, 1963), 71-162.

each of certain vocational groups of the upper Third Estate and suggest that the members of each vocational category may have been distributed among two or more degrees of status.⁸ But the fundamental question is certainly whether the bourgeoisie of 1789, however defined, had any economic consistency that opposed it to other classes grounded in different forms of wealth. This paper has to do with distinctions between capitalist and non-capitalist wealth and what these imply about the revolution of the upper Third Estate, the movement that began with the demand for doubling the representation of the Third Estate and voting by head rather than by order. It also offers a way of explaining that revolution without having recourse to the present terminology.

There was in the economy of the old regime a distinct configuration of wealth, noncapitalist in function, that may be called "proprietary." It embodied investments in land, urban property, venal office, and annuities. The returns it yielded were modest, ranging between 1 and 5 per cent, but they were fairly constant and varied little from year to year. They were realized not by entrepreneurial effort, which was degrading, but by mere ownership and the passage of calendar intervals. Risk was negligible. Although bad harvests lowered rents in kind, they never destroyed capital, and the rents in money, like annuities and salaries of venal office, were payable regardless of natural hazards. In the proprietary sector investments were almost fully secure.⁹

Historically and functionally, proprietary wealth was aristocratic. Under the old regime, gentility required a stable fortune that left one free to live with ease and dignity on his revenues. In the fortunes of the Toulouse nobles studied by Forster¹⁰ and of the magistrates of the Paris Parlement studied by Bluche¹¹ it was precisely land, urban property, venal office, and annuities that furnished the income on which these families maintained their way of life. Two considerations discouraged nobles from investing in commerce. First,

⁸ The dispersion of wealth and income within single vocational categories and juridical orders appears in Adeline Daumard and François Furet, *Structures et relations sociales à Paris au XVIII^e siècle* (Paris, 1961), 18–19; Michel Vovelle and D. Roche, "Bourgeois, rentiers, propriétaires: Éléments pour la définition d'une catégorie sociale à la fin du XVIII^e siècle," in *Actes du Quatre-Vingt-Quatrième Congrès National des Sociétés Savantes (Dijon, 1959)* (*Section d'Histoire Moderne et Contemporaine*) (Paris, 1960), 419–52; and François Dornic, *L'industrie textile dans le Maine et ses débouchés internationaux (1650–1815)* (Le Mans, 1955), 164–68.

⁹ Not that risk was entirely absent; it was simply remote, particularly when compared with commercial risk. Houses and forests could burn. A *rente* might go unpaid, and the *rentier* might have to sue for it.

¹⁰ Robert Forster, *The Nobility of Toulouse in the Eighteenth Century* (Baltimore, 1960), 47–119.

¹¹ François Bluche, *Les magistrats du parlement de Paris au XVIII^e siècle (1715–1771)* (Paris, 1960), 143–239.

the social values of aristocracy included a notorious aversion to business as practiced by merchants, merchant manufacturers, and bankers. To invest in "trade" was to risk losing status. The only industries that nobles felt entirely free to develop were those rooted in the land and its resources and growing out of certain exploitations of the medieval fief—mines, metallurgy, paper, glass, and canals—and in developing these they adopted practices and forms of organization substantially different from those employed by the merchants.¹² Second, the risks inherent in business endangered the solidity and continuity considered essential to wealth meant to support a family for several generations. Fundamentally, the fortune that best served the interests of an aristocratic family was an endowment. Like an endowment, it was carefully managed, and risk was kept to a minimum. The preference for this kind of wealth, based on ingrained social attitudes that have powerfully retarded French economic growth, survived the Revolution. When the Napoleonic aristocracy was established, a landed endowment or *majorat* was required of anyone raised to the peerage, and he and his heirs were forbidden to alienate it except in exchanges of land.¹³ Both before and after the Revolution, the social values of the old elite dominated the status-conscious men and women of the wealthy Third Estate. Avid for standing, they had little choice but to pursue it as the aristocracy defined it, and the result was a massive prejudice that diverted *roturier* as well as noble wealth into comparatively sterile proprietary investments.

In describing this system of wealth, the word "proprietary" does better than "feudal." Cobban has pointed out that, in terms of property, "feudalism" could refer only to the seigneurie.¹⁴ The seigneurie, consisting of dues, monopolies, and rights surviving from the fief, was an order of property superimposed on property in fee simple, and it could be and was acquired by nonnobles. But seigneurial rights figured marginally in a larger preference for all long-term assets yielding secure revenues and standing, a taste for "property" in every form, not only seigneuries but domains, farms, *métairies*, meadows, fields, stands of timber, forges and mills that could be rented out, houses, buildings, venal offices, and loans of indefinite duration producing annuities called *rentes perpétuelles*.¹⁵ Such properties were enduring. Combined into endowments yielding assured revenues, carefully managed, they

¹² Taylor, "Types of Capitalism," 495–96.

¹³ Napoleon I to Joseph Bonaparte, king of Naples, June 5, 1806, in *Mémoires et correspondance politique et militaire du roi Joseph*, ed. Baron Albert du Casse (2d ed., 10 vols., Paris, 1854–55), II, 274.

¹⁴ Cobban, *Social Interpretation*, 25–35.

¹⁵ In a later section the *rente perpétuelle* will be treated in more detail.

could be made to support a family indefinitely in a genteel style of living.¹⁶ They guaranteed a status, by no means exclusively noble, that Professor Palmer has called aristocratic.¹⁷ The term "proprietary" describes these fortunes not only because it is derived from "property" but also because it echoes the old regime term *propriétaire*, a prestige counter claimed by those who owned land, even in trifling amounts.¹⁸

The fondness for land penetrated all levels of French society. Profoundly rural, most eighteenth-century Frenchmen had an atavistic attachment to the soil, and "living nobly" was habitually identified with at least seasonal residence in the country. The aristocracy by tradition and the wealthy urban groups by emulation showed an incurable esteem for rural property. The novelist Stendhal, raised in the 1780's at Grenoble, recalled his father, an *avocat au parlement*, as a man constantly preoccupied with acquiring rural land and expanding his holdings. His father's wigmaker (*perruquier*), on missing an appointment with a client, would explain that he had been visiting his "domain," and his excuses were well received. People bought land yielding 1 or 2 per cent with funds that could have been deposited with merchants at 5, and borrowed at 5 to buy land that yielded 1 or 2.¹⁹ This passion for land was by no means limited to Grenoble. Nobles, *avocats*, *procureurs*, financiers, officials, and merchants in all parts of France bought and held urban and rural properties that qualified them for local acceptance, advancement, and privileges.²⁰ There were shopkeepers, artisans, and even

¹⁶ The worst threat to the sound management of such a fortune was an improvident head of the family. Aside from this, the most difficult problem to solve was that of paying out bequests and dowries over a period of years in such a way as not to dissipate the capital. (Forster, *Nobility of Toulouse*, 120-77.)

¹⁷ R. R. Palmer, *The Age of the Democratic Revolution* (2 vols., Princeton, N. J., 1959-64), I, 29-30.

¹⁸ In 1789 the *maîtres marchands fabricants* of Lyons asked for an electoral representation of their own, apart from that of the *Grande Fabrique* of the local silk industry, in which they were outvoted by the *maîtres ouvriers*. One of their arguments was that most of them were *propriétaires*. (Archives Nationales [hereafter cited as AN], B III, 75.) The word occurs often in registrations of Third Estate deputies for the Estates-General, and it was obviously a status counter for *roturiers* who, now in the presence of the *grande noblesse*, needed all the prestige they could get. Seven deputies listed themselves simply as *propriétaires*. Both an *avocat au parlement* and a physician added the word *propriétaire* to the terms that designated their professions. One deputy called himself *bourgeois et propriétaire*, and another *bourgeois, propriétaire, et seigneur*. There was a countryman, probably a *fermier* with a few arpents of his own, listed as a *propriétaire-cultivateur*. Another called himself *laboureur et propriétaire*—a redundancy, since *laboureurs* were by definition owners of land. The term *seigneur* was, of course, still more prestigious, indicating, as it did, a traditionally noble kind of property. It was claimed by a director of the mint, a municipal officer (*ammeistre*), two *procureurs*, seven magistrates of the *bailliages* and *sénéchaussées*, two *avocats au parlement*, one *avocat*, an *ancien mousquetaire*, a *bourgeois*, and four plain old *seigneurs*. (*Les Constituants: Liste des députés et des suppléants élus à l'Assemblée Constituante de 1789*, ed. Armand Brette [Paris, 1897], 3-193 et passim.)

¹⁹ Stendhal [pseud. of Henri Beyle], *The Life of Henri Brulard*, tr. Catherine Alison Phillips (New York, 1955), 77-78, 150, 214.

²⁰ Some documentation on rural and urban property acquired by merchants, both before and after the Revolution, is found in materials on sequestrations of the Year II at Lyons (Archives

peasants who invested in land and *rentes* that gave them small incomes for old age.²¹ In every town those without a business or profession who lived on such investments were taxed on a separate roll, that of the bourgeois, and in 1789 in most towns they voted as a separate electoral group of the Third Estate. A study by Vovelle and Roche shows that the qualification bourgeois disappeared during the Revolution from official acts, and that persons listed under the old regime as bourgeois reappeared in documents of the Directory and the Consulate as *rentiers* and *propriétaires*,²² demonstrating as well as anything can that before the Revolution the fiscal group called bourgeois was noncapitalist.

Nearly all wealthy landowners exploited their land indirectly, through tenants.²³ They saw their properties not as profit-making enterprises but as sources of rental income. Rent, in fact, was at the center of all calculations. It was what determined the value of a property: as rent increased, the value grew proportionately, so that, curiously enough, the rate of return on capital remained about the same.²⁴ Generally speaking, rental income seems to have ranged between 2 and 4 per cent of capital value, and Necker wrote in 1784 that the net revenue from land was 2½ per cent,²⁵ which is to say that, as an investment, land provided the low but assured return typical of proprietary wealth. When an eighteenth-century proprietor set out to increase the rev-

du Rhône, Ser. 1Q and 2Q), testimony of merchants tried by the *Commission Militaire de Bordeaux* (Archives de la Gironde, 12L, 1-37), and declarations at Toulouse for the forced loan of the Year II (Archives Municipales de Toulouse, old cote G², liasses 2, 3, 42, 43, 44, 94). At Elbeuf in 1780 a third of all taxpayers owned real property. A sampling of wills made by "manufacturers," 1792-1806, shows that they left 5.3 per cent of their fortunes in personal effects, 26.5 per cent in *rentes*, and 68.2 per cent in real estate; and for the merchants the corresponding percentages would be 13.3, 22.6, and 64.1. (Jeffrey Kaplow, *Elbeuf during the Revolutionary Period: History and Social Structure* [Baltimore, 1964], 88-91.) The propensity of these entrepreneurs of Elbeuf for acquiring urban and rural property is typical at least of the region. (See, e.g., Pierre Dardel, "Une famille de manufacturiers en Haute-Normandie: Les Pouchet (xv^e et xix^e s.)," *Annales de Normandie*, XII (Nos. 2, 3, 1962), 93-107, 169-84.

²¹ Michel Vovelle, "Structure et répartition de la fortune foncière et de la fortune mobilière d'un ensemble urbain: Chartres de la fin de l'ancien régime à la Restauration," *Revue d'histoire économique et sociale*, XXXVI (No. 4, 1958), 385-98.

²² Vovelle and Roche, "Bourgeois, rentiers, propriétaires," 437.

²³ Georges Lefebvre, *Questions agraires au temps de la Terreur* (2d ed., La Roche-sur-Yon, n.d.), 58. For figures on certain regions, see Jean Loutchisky, *L'état des classes agricoles en France à la veille de la Révolution* (Paris, 1911), 57-58.

²⁴ Methods of land evaluation and their importance for historical research are discussed in D. Zolla, "Les variations du revenu et du prix des terres en France au xvii^e et au xviii^e siècle," *Annales de l'École Libre des Sciences Politiques*, VIII (No. 2, 1893), 299-308.

²⁵ Jacques Necker, *De l'administration des finances de la France* (3 vols., Paris, 1784), III, 238. In Burgundy the return on land ranged from 2 to 4 per cent. (Pierre de Saint-Jacob, *Les Paysans de la Bourgogne du nord au dernier siècle de l'ancien régime* [Paris, 1960], 499-500.) In 1789 Arthur Young was offered for sale two estates yielding revenues at 2½ per cent and 4 per cent of the asking price. (Arthur Young, *Travels in France during the Years 1787, 1788, & 1789*, ed. Constantia Maxwell [Cambridge, Eng., 1950], 203-206.) At Orléans, one De Champvallin fils, who owned country properties in four parishes valued collectively at 231,000 livres, realized 6,000 livres on them, for a return of 2.6 per cent. (Georges Lefebvre, *Études orléanaises* [2 vols., Paris, 1962-63], I, 147.)

enue of his properties he thought not in terms of increasing the productivity of the soil but of raising the rent, and in the late eighteenth century a significant rise of the peasant population made this easy to do. As land hunger grew and candidates for leases multiplied, rents rose handsomely. Labrousse has found that, from the base period 1726-1741 to the "intercyclic" period 1785-1789, rural money rents advanced by 98 per cent, and in a paper on the royal domains submitted to the Assembly of Notables in 1787 mention was made of "the Revolution which, in twenty years, has nearly doubled the revenues of all land."²⁶ Where the rent was paid in kind, as in *métayage*, the rise in rents is difficult to measure, but there is no doubt that it took place. Forster has written that in the Toulouse region the old phrase "half-fruits" that signified the owner's share was a euphemism; at the end of the century the owners took as much as three-fourths.²⁷ It is perhaps possible to say that the French landowner of the old regime was an exploiter of persons rather than of the soil. The circumstances of the prerevolutionary period did nothing to change his traditional attitudes. Indeed, by enabling him to raise his income without raising production they reinforced them.

If in the eighteenth century France had had an agricultural revolution comparable to that in England, it would be possible to speak of agricultural capitalism and to discover an entrepreneurial mentality that saw income as profit and was prepared to increase profits by investing in productivity. Unfortunately for the old regime, no such thing took place. There was, of course, much interest among certain upper-class intellectuals in British agricultural innovations, and, beginning in 1750, there began to appear a large body of publications on the subject. Agricultural societies were formed, and experiments were undertaken.²⁸ Nevertheless, the results were meager. The peasants distrusted innovations and sabotaged experiments, and proprietors who wished to install improvements failed because, in order to succeed, they would have had to learn to work and think like peasants, which was exactly what their values prevented them from doing.²⁹ But it was not only the

²⁶ C.-E. Labrousse, *Esquisse du mouvement des prix et des revenus en France au XVIII^e siècle* (2 vols., Paris, 1933), II, 417-46. The quotation from the paper on the royal domains is offered from the procès-verbaux of the Assembly by Jean Egret, *La pré-révolution française (1787-1788)* (Paris, 1962), 49.

²⁷ Forster, *Nobility of Toulouse*, 56-58.

²⁸ A. J. Bourde, *The Influence of England on the French Agronomes, 1750-1789* (Cambridge, Eng., 1953).

²⁹ The minutes of the Committee of the Administration of Agriculture, which met at Paris during 1785-1787, testify overwhelmingly to the failure of French attempts to emulate the British agricultural revolution. (*L'administration de l'Agriculture au Contrôle Générale des Finances (1785-1787): Procès-verbaux et rapports*, ed. Henri Pigeonnet and Alfred de Foville [Paris, 1882].) Forster, having studied noble landownership around Toulouse, Bordeaux, and Rennes, concludes that, "Efforts of the noble landlord to alter crop courses, plant forage crops, and improve the livestock in the contemporary English manner were rare." (Robert Forster, "The Provincial Noble: A Reappraisal," *American Historical Review*, LXVIII [Apr. 1963], 686.) Alun Davies, in an article on "The New Agriculture in Lower Normandy,

disinterest of landowners or their unfitness to provide leadership that aborted the French agricultural revolution. There were many other obstacles which, taken together, would have defeated the boldest plans of agricultural reform: the fragmentation of domains into small, dispersed parcels of property; the stubbornness with which country people defended common rights and broke down enclosures; the burden of the *taille*, which penalized initiative; the hostility of peasants to new crops, crop courses, and methods of cultivation; the tyranny of the leases, which fixed the crop courses in the old patterns; and finally the shortage of livestock, which assured a shortage of manure, which assured a shortage of improved meadows, which in turn assured a shortage of livestock.³⁰ Given all these barriers to improvement, the proprietary mentality, with its noncapitalist orientation, was not terribly unrealistic. In 1788 the scientist-financier Lavoisier, a careful student of agriculture, told the provincial assembly of the Orléanais that the productivity of British agriculture was about 2.7 times that of French agriculture and that the capital invested per unit of land was two or three times greater in Britain than in France. He owned an experimental farm. In four years he had invested 120,000 livres in it. In his judgment, which would seem well established, the improvements needed to raise productivity in the Orléanais would require much more of an outlay than the proprietors could or would invest.³¹ All these deterrents—legal, psychic, and social—checked economic growth and increased the danger of breakdowns like that of 1788–1790, which in its origins was largely, though not wholly, agrarian³² and unleashed the rural and urban disorders of the common people, disorders without which the Revolution of 1789 could not have succeeded.

1750–1789" (*Transactions of the Royal Historical Society*, 5th Ser., VIII [1958], 129–46), actually explains how the agricultural revolution was thwarted there. On the incapacity of partisans of agrarian reform to apply the new agriculture for lack of practical experience in cultivation, see Young, *Travels in France*, ed. Maxwell, 128, 201, 242. An authoritative verdict is that of Georges Bourgin: "In my opinion," he wrote, "what characterizes the eighteenth century from the agricultural point of view is the disproportion that existed between the programs and hopes on the one hand, and the results on the other." (Quoted by Bourde, *Influence of England*, 218.)

³⁰ On the struggle to liberate cultivation from the obstacle of common rights, see Marc Bloch, "La lutte pour l'individualisme agraire dans la France du XVIII^e siècle," *Annales d'histoire économique et sociale*, II (Nos. 7, 8, 1930), 329–84, 511–54. The peasants, however, resisted the movement, and from 1771 onward the intendants opposed enclosures because they led frequently to disturbances of the peace. (*Id.*, *Les caractères originaux de l'histoire rurale française* [Paris, 1931], 226; Saint-Jacob, *Paysans de la Bourgogne du nord*, 369–71.)

³¹ Antoine-Laurent Lavoisier, "Sur l'agriculture et le commerce de l'Orléanais," *Oeuvres de Lavoisier, publiées par les soins du ministre de l'instruction publique*, ed. Édouard Grimaud (6 vols., Paris, 1862–93), VI, 255–85. Lavoisier was also a member of the Committee of the Administration of Agriculture and took an active part in its proceedings.

³² Labrousse, *Esquisse*, II, 514–67, and again with more insistence in his *La crise de l'économie française à la fin de l'ancien régime et au début de la Révolution*, I, *Aperçus généraux, sources, méthode, objectifs, la crise de la viticulture* (Paris, 1944), xxix, 173, 175–76; demurrals by David S. Landes, "The Statistical Study of French Crises," *Journal of Economic History*, X (No. 2, 1950), 195–211.

In the proprietary scale of preference, the passion for property in office was nearly as strong as that for property in land. A venal office was a long-term investment. Usually it brought a low but stable return, and, as long as the owner regularly paid the *droit annuel* (in earlier times the *paulette*), he could, under restrictions applicable to each office, sell it to a buyer, bequeath it to an heir, or even rent it out to someone, such as a judge, who, though admitted to practice, was unable to buy the required *charge*.³³ The number and variety of venal offices that existed at the end of the old regime is incredible. An investigation that Necker launched in 1778 disclosed no less than 51,000 venal offices in the law courts, the municipalities, and the financial system, and their capital value, as revealed by voluntary declarations made under an edict of 1771, totaled 600,000,000 livres, although this should be increased by as much as 50 per cent because the declarations, taxable at 1 per cent per annum, were notoriously undervalued.³⁴ These offices included those held by the personnel of the parlements and their chancelleries, the judges of the other royal courts, and the multitude of clerks, beadles, sergeants, surveyors, assessors, and concessionaires that surrounded these courts. They also included the offices held by the notaries and *procureurs*, who could practice their professions only by acquiring the appropriate *charges*. They did not, however, include the offices of the royal household, venal military appointments, or places in the financial companies and the higher financial concessions like those of the *receveurs généraux des finances*, and for these we should probably add another 200,000,000 or 300,000,000 livres to the total indicated above. Also excluded from these figures were the offices held by guild officials, inspectors, and masters, and particularly by the wigmakers.³⁵ Given the present state of research we have no precise idea of how many adult males owned offices, but it would not be surprising to find that they came to 2 or 3 per cent of the total.

Ordinarily, the declared value of an office was only part of the cost of buying and exercising it. Nearly always, it was sold for a price higher than that

³³ *Encyclopédie méthodique: Finances* (3 vols., Paris, 1884-87), articles: "Annuel," I, 45-46; "Casuelles," I, 209; "Centième denier," I, 218-19; "Charge," I, 244-46; "Maison du roi," III, 2-12; and "Office," III, 244-61.

³⁴ Reports and project of decree on reimbursement of offices suppressed by decrees of Aug. 4, 11, 1789, presented by Pierre-François Gossin, deputy of the Third Estate of Bar-le-Duc, in the name of the *Comité de Judicature* to the National Assembly, Sept. 2, 1790, *Archives parlementaires de 1787 à 1860 . . .*, 1st Ser., ed. Jean Mavidal *et al.* (86 vols., Paris, 1867-1965), XVIII, 494-510.

³⁵ *Ibid.*, 501. In the liquidation decrees the offices of the wigmakers presented a special problem because since 1771 their value in sale had risen exceptionally. Evaluated at Lyons at five thousand livres, they sold there for from twelve thousand to fifteen thousand livres. At Angers the declarations were for one thousand livres, and the selling price was for four thousand or five thousand. (Report of Claude-Ambroise Régnier, deputy of the Third Estate of Nancy, to the National Assembly, June 17, 1791, *ibid.*, XXVII, 285.) There is a list of some of the petty offices, the small change of venality, in *Encyclopédie méthodique: Finances*, I, 244.

recorded in the declarations and contracts, and the investment was increased by heavy taxes, fees for registration and reception, and the honoraria, gratuities, and *pourboires* that a candidate had to distribute to officials, clerks, beadles, and even doormen in obtaining his nomination. Philip Dawson has brought to light the case of a young *avocat* who in 1781 purchased a magistracy in the *sénéchaussée* of La Rochelle. In the contract of sale, filed with a notary, the price was put at 10,167 livres. But the buyer's notes show that he really paid 14,000 livres, plus another 4,150 in taxes, dues, fees, and gratuities, all of which means that the total investment exceeded the stated value of the office by 78 per cent.³⁶ When an office gave admission to a profession, the disproportion between its acknowledged value and the full investment was apt to be still greater. In March 1787 the future revolutionary Danton bought the office of *avocat au Conseil du Roi* for 10,000 livres; at the same time, however, he paid the seller 68,000 livres for his practice, including the clientele and the accounts receivable.³⁷ In short, the additional costs and professional outlays that accompanied investments in venal office raised the total French private funds committed to this purpose far above the more than 600,000,000 livres with which the Revolution compensated those whose offices it abolished.

Few venal offices were genuinely lucrative. On the 51,000 judicial, municipal, and financial offices covered by the 1778 investigation, the salaries, after deducting for the *droits annuels*, *vingtièmes*, and transfer taxes (*droits de mutation*), averaged only 1 per cent of the values declared in 1771, although in most cases there were fees, perquisites, and gratuities that made up the interest on the declared capital.³⁸ Whatever economic value they had depended on how the owners used the opportunities that accompanied them. For a notary or *procureur*, the income from a practice could constitute a very decent return on the total outlay. But for the magistrates it was likely to be a different story. Although it was taken for granted that a Parisian *conseiller au parlement* cleared 5 per cent per annum on his investment, the *présidents à mortier* made only 2 per cent, and the First President, burdened with costs of maintaining the dignity of his position, probably spent more than he received.³⁹ Generally speaking, an investment in office was an investment in standing. What made it desirable was the status, the respectability that it

³⁶ Philip Dawson, "Sur le prix des offices judiciaires à la fin de l'ancien régime," *Revue d'histoire économique et sociale*, XLII (No. 3, 1964), 390-92. I am indebted to Professor Dawson for having taken the trouble to clarify for my benefit the relation between venal office and admission to the legal profession.

³⁷ Georges Lefebvre, "Sur Danton," in his *Études sur la Révolution française*, ed. Albert Soboul (2d ed., Paris, 1963), 64-65.

³⁸ Report of Gossin, 499.

³⁹ Bluche, *Magistrats du parlement de Paris*, 169.

conferred. For a solid gain in prestige, the holders of *charges* would usually settle for a low return and even a loss of capital. In 1790, for example, the National Assembly was told that the magistrates of some parlements, by excluding *roturiers* from admission, had so narrowed the market for their offices as to reduce the purchase price from more than 50,000 livres to as little as 15,000.⁴⁰ In effect, they sacrificed capital for status, which was not unnatural in a society afflicted with a mania for prestige. Apparently, it meant a great deal to be a *lieutenant-civil*, a *lieutenant-criminel*, a *procureur du roi*, a *grand-maitre des eaux et forêts*, or even a *conseiller au grenier à sel*. To the *roturiers* it meant still more to acquire an office that gave noble rank. According to Necker, there were more than four thousand of these,⁴¹ although perhaps half of them, like the magistracies of the parlements, were inaccessible to commoners. For example, a *secrétaire du roi* was ennobled by his office and, if he held it twenty years or died possessed of it, acquired *noblesse transmissible* for his heirs and descendants. A *trésorier de France* enjoyed *noblesse personnelle*; although his family did not share this, there was nothing to prevent him from bequeathing his office to his eldest son, and it appears that some of these offices gave *noblesse transmissible*. According to Necker, there were 900 *secrétaires du roi* attached to the chancelleries of the parlements and 740 places that one takes to be those of the *trésoriers de France* in the financial apparatus. Nothing indicates that the propensity of these offices for creating new nobles had been cut off at the end of the old regime.

In addition to land, urban properties, and office, proprietary wealth was invested in *rentes*. In the broadest sense, a *rente* was an annual revenue that one received for having transferred something of value to someone else. A *rente foncière* was rent for land. A *rente hypothécaire* was an annuity the payment of which was secured by property. A *rente perpétuelle* was an annuity of indefinite duration, terminated only when the debtor chose, on his own initiative, to refund the principal and thereby free himself from paying the *rente*. A *rente viagère* was a life annuity: the principal was entrusted to someone who paid the annuity until the person or persons named in the contract died; at that point the principal became finally and irrevocably his.⁴² Because the *rente viagère* was essentially a speculation that destroyed all or part of the capital accumulated for a family endowment, most of those living on proprietary wealth believed it reckless and immoral, and a man who

⁴⁰ Report of Gossin, 497.

⁴¹ Necker, *De l'administration des finances*, III, 145-46. Research on the juridical background of ennoblement by office is considerably advanced by the work of François Bluche and Pierre Durye, *L'noblesse par charges avant 1789* (2 vols., La Roche-sur-Yon, 1962).

⁴² *Encyclopédie méthodique: jurisprudence* (10 vols., Paris, 1782-89), VII, 305, 314-15.

converted his fortune into life annuities was considered to have defrauded his heirs.⁴³

To an American student, the rationale of this vocabulary is elusive. Everything is clarified, however, by the fact that the vocabulary took shape during the late Middle Ages, when those who wished to borrow, and those who wished to lend, had to find ways of disguising loans at interest so as to circumvent the laws against usury. The terminology of the *rente* made this possible, at least during the sixteenth and seventeenth centuries. One spoke, for example, of purchasing a *rente*: this modulated the smell of avarice and exploitation by making it seem that the lender, who bought the *rente*, had solicited it from the borrower, who sold it, and obtained it on the borrower's terms. The vocabulary also improved appearances by assimilating all these transactions to land rents, which were undoubtedly on the right side of the law.⁴⁴ Schnapper has shown that the *rente perpétuelle* began as an annual rent paid to a seller of land by a buyer who could not furnish the full price and, in effect, paid rent on that part of the property he did not own. In the sixteenth century, however, the *rente perpétuelle* acquired an existence apart from real-estate transactions. It then became a perpetual annuity paid for a grant of capital that an investor (the lender) "abandoned,"⁴⁵ and this was its legal character through the end of the old regime. In the contract, whatever the parties may have agreed verbally or in separate instruments, to stipulate a time of repayment was forbidden, and no such stipulation could be enforced in the courts, for the Church insisted upon a permanent alienation of the capital. Only the borrower, the "seller" of the *rente*, could decide whether the capital would be restored, and, if so, when. No doubt, if he failed to pay the *rente* he could be forced into ceding property that might equal or even exceed the original capital, and perhaps there were other pressures that creditors could employ. Whatever the truth may have been, it seems probable that in the eighteenth century many borrowers gave assurances that the capital would be repaid at a stipulated time. Those who lived up to these assurances probably did so in large part so as to protect their credit and reputations.

The indefinite duration of the *rente perpétuelle* ruined it for commerce, industry, banking, and the short-term credits that the financiers furnished the royal treasury. In these sectors, advances at interest were indispensable,

⁴³ Printed prospectus, "Remboursement de Capitaux, assurés à l'extinction des Revenus viagers & autres Usufruits," attributed to the banker Isaac Panchaud, AN, F¹² 798^c.

⁴⁴ From the reign of Francis I, when *rentes* were first constituted at the Paris Hôtel de Ville, the nobles of the Paris Parlement showed no repugnance at investing in them. (Bluche, *Magistrats du parlement de Paris*, 212.)

⁴⁵ Bernard Schnapper, *Les rentes au xvi^e siècle: Histoire d'un instrument de crédit* (Paris, 1957), 63.

and, although they were nominally illegal, the parties were shielded from prosecution by a national conspiracy, abetted by the administration, to keep the usury laws from being invoked.⁴⁶ Merchants gave and took interest on the balances of their accounts with one another and paid interest on time deposits put up by investors. Bankers took interest for many kinds of accommodations.⁴⁷ The King, himself, violated the usury laws, and there was even a royal rate of interest which, since the time of Louis XIV, was fixed at 5 per cent.⁴⁸ Under these circumstances it is difficult to see why the archaic *rente perpétuelle* survived. It survived, of course, because it met most of the demands for long-term credit operations in the traditional or proprietary sector, where there were few pressures for collection and payment and where people took satisfaction in avoiding the questionable practices of *traitants* and *commerçants*. Its proper domain was that of accommodations within and between families and investments in annuities sold by municipalities, provincial estates, and the royal treasury. Although economically obsolete, it not only survived, but left a mark on the management of royal and private wealth. Among other things, it engendered that characteristic insouciance toward debt for which the old regime was famous and induced a dangerous negligence in royal finance. "The abundance of claims and credits of indefinite duration," writes Schnapper, "is a characteristic trait of old economies. The creditor prefers a fixed revenue to a capital for which he cannot easily find use. The debtor, himself, never repays because he does not have sufficient monetary means."⁴⁹ It is impossible to read this without thinking of the old French royal debt and the bankruptcy in which it finished. On January 1, 1789, the registers of the Paris Hôtel de Ville showed 52,119,537 livres in *rentes perpétuelles* to be paid out during the year; of this more than 44 per cent represented annuities on funds borrowed before 1721.⁵⁰ In large part, the chronic and ultimately fatal disinclination to amortize the long-term debt is attributable to the fact that the capital of a *rente perpétuelle* did not have to be repaid. Neither, of course, did that of a *rente viagère*. It was therefore easy to drift into bankruptcy. Only when service on the long-term debt was so large as to make deficits inescapable would a controller general have

⁴⁶ As demonstrated by the role played by Turgot and the King's Council in connection with the usury scandal at Angoulême in 1769. Anne-Robert-Jacques Turgot, Baron de l'Aulne, *Oeuvres de Turgot et documents le concernant*, ed. Gustave Schelle (5 vols., Paris, 1913-23), III, 154-204.

⁴⁷ As shown by the ledgers of the Banque Mallet de Paris, 1770-1793 (incomplete), AN, 57 AQ, 1-10; and the books and papers of Veuve René Guérin et fils (Saint-Chamond), later the Banque Guérin, Archives du Rhône, Ser. 2F.

⁴⁸ Samuel Ricard, *Le praticien des juges et consuls* (new ed., Paris, 1742), 455; *Encyclopédie méthodique: Commerce* (3 vols., Paris, 1783-84), II, 729-30.

⁴⁹ Schnapper, *Rentes au xvi^e siècle*, 63.

⁵⁰ "Tableau des rentes perpétuelles de l'Hôtel-de-Ville de Paris," statement dated Jan. 1, 1789, *Archives parlementaires*, ed. Mavidal et al., XI, 392.

to consider refunding principal, but then, of course, he would find it impossible to pay. That was precisely the quandary of the controllers general of finance after the American war.

It should now be clear that there was a fairly consistent pattern of non-capitalist wealth, that it was traditionally aristocratic, and that "feudalism" is a bad name for it. It was governed by institutional survivals and social values that opposed the progressive and expansive tendencies of capitalism, preferring rent to profit, security to risk, tradition to innovation, and, in terms of personal goals, gentility to entrepreneurial skill and renown. It displayed nearly all the traits of what Rostow has called a traditional society, one dominated by landowners and their values and governed, as far as production was concerned, by pre-Newtonian modes of thought. All these institutions, values, and fixations promoted, as Rostow has suggested, a "long-run fatalism" and a "ceiling on the level of obtainable output per head."⁵¹ In England, no doubt, such deterrents to growth existed, but in ways that are not yet clearly explained they were being outflanked or overcome. In France, however, they flourished. The question of why there should have been such a disparity deserves much more study than it has received.⁵²

Compared with proprietary wealth, eighteenth-century commercial capitalism seems a vastly different thing.⁵³ In commerce, banking, and domestic industry fixed assets were negligible, and investments were put into circulating wealth. Goubert has written of the Motte family of Beauvais:

One is tempted to write that what was always important to those merchants-born [*marchands-nés*] was wealth in motion, the rather intoxicating impression that must have come to them from the merchandise, credits, and cash that moved, circulated, fluctuated, and constantly transformed themselves: a kind of ballet of linens, paper, and money.⁵⁴

This engaging description of commercial wealth is justified by entrepreneurial

⁵¹ W. W. Rostow, *The Stages of Economic Growth: A Non-Communist Manifesto* (Cambridge, Eng., 1960), 4-7. The idea, which Rostow considers essential to growth, that economic progress is possible and necessary was entrenched in the more imaginative and responsible circles of royal administration and certain clusters of intellectuals who tried to influence public policy. But the reformers were frustrated by inalterable privileges and concessions. That is essentially what blocked the abolition of internal tariffs. (See the excellent study of J. F. Bosher, *The Single Duty Project: A Study of the Movement for a French Customs Union in the Eighteenth Century* [London, 1964].)

⁵² But see David S. Landes, "Technological Change and Development in Western Europe, 1750-1914," *The Cambridge Economic History of Europe*, ed. H. J. Habakkuk and Michael M. Postan (6 vols., Cambridge, Eng., 1941-65), VI, 274-601, esp. 274-421; and François Crouzet, "Angleterre et France au XVIII^e siècle: Essai d'analyse comparée de deux croissances économiques," *Annales: Économies, sociétés, civilisations*, XXI (Mar.-Apr. 1966), 254-91.

⁵³ Taylor, "Types of Capitalism," 481-87, and "Some Business Partnerships at Lyon, 1785-1793," *Journal of Economic History*, XXIII (Mar. 1963), 46-70.

⁵⁴ Pierre Goubert, *Familles marchandes sous l'Ancien Régime: Les Danse et les Motte, de Beauvais* (Paris, 1959), 37.

records in many archives. At Lyons merchants rented the houses and warehouses in which they did business. With the *armateurs* of Bordeaux and Marseilles, ships were short-term assets; bought by a syndicate organized to finance the voyage, the ship was sold at the end of the venture, sometimes at auction, sometimes simply to the syndicate the *armateur* had formed for the next voyage. Industrial machinery was simple and made mostly of wood. In textiles, which accounted for about two-thirds of industrial production by value, it was owned chiefly by the artisans to whom the work was distributed, and when merchants loaned it to them it was not serious enough to warrant carrying in the accounts. All this explains why the ledgers of the old regime carry no accounts for depreciation costs.⁵⁵ The day of heavy fixed commercial and industrial investment was yet to come.⁵⁶

Risk, nearly unknown in the proprietary sector, was a central fact of business life. The merchant speculated in commodities, paper, and credit, and, no matter how prudent he was, his fate depended largely on events he could not control. Shipwrecks, acts of war, sudden changes in style, unforeseeable bankruptcies, or unfavorable shifts in exchange rates could wipe him out, and if it was bad luck that broke him it was largely good luck that made him rich. Established merchants, known for caution and probity, went under, while new men, starting with borrowed money and the savings of a clerk's salary,⁵⁷ became well to do. Commerce, therefore, was a zone of fortune building and social mobility. But because it lacked the stability of the proprietary sector, it was dangerous for established wealth.⁵⁸ "All that I have seen," wrote the Comte de Villèle, ". . . leaves me with the opinion that every man with an acquired fortune who desires only to keep it, must keep at a distance from people, of whatever class or profession they be, who strive to make a fortune; . . ."

he must avoid all business, all relations with them, because they will not fail to make him their dupe. Furthermore, to each man his *métier*, as the proverb says:

⁵⁵ Taylor, "Types of Capitalism," 483. In the eighteenth-century manuals on bookkeeping there is no awareness of depreciation as a problem. (C.-F. Gaignat de l'Aulnais, *Guide du commerce* . . . [Paris, 1791]; Pierre Giraudeau l'aîné, *La banque rendue facile aux principales nations de l'Europe* [last ed., Lyons, 1793]; De la Porte, *La science des négocians et teneurs de livres* . . . [new ed., Amsterdam, 1787].)

⁵⁶ And for that reason the need for high fixed capital investment began to affect entrepreneurial forms only after the end of the revolutionary period. (Bertrand Gille, *Recherches sur la formation de la grande entreprise capitaliste (1815-1848)* [Paris, 1959], 163.)

⁵⁷ The upward mobility of the clerks, and their opportunity to save capital for entrepreneurial investment, is discussed in Taylor, "Some Business Partnerships at Lyon," 55-56. An example of such a beginning was that of the banker Jacques Laffitte, son of a carpenter, who started with nothing as a clerk with a maritime underwriter of Bayonne. (*Mémoires de Laffitte*, ed. Paul Duchon [Paris, 1932], 1-50.)

⁵⁸ Although a special form of partnership, the *société en commandite*, offered limited liability to one or more partners, few nonmerchants showed any inclination to involve themselves in such enterprises.

look at the proprietor trying to speculate, and at the merchant trying to enter agriculture. . . . Never have I participated in the least speculation.⁵⁹

Finally, in contrast to proprietary wealth, business capital gave low dividends in prestige. The public image of the merchant that Molière exploited rather brutally in *Le bourgeois gentilhomme* was profoundly ignoble, and it afflicted the merchants themselves with feelings of inferiority that probably troubled them more than the contempt they actually encountered. To some extent their unhappiness was self-induced. In 1700–1701 merchant deputies to the Council of Trade complained that merchants were held in low esteem, that the public ignored the superior status of a wholesale merchant or banker, and that because of this their sons avoided business and their daughters preferred nonmerchants as husbands. “Our young people,” wrote one of them, “concentrate on the social graces rather than on the really substantial things in life, [and] our children are ever fearful lest it become known that their fathers were once merchants.”⁶⁰ About thirty years before the Revolution the Abbé Coyer wrote: “Only the Merchant perceives no luster in his career, & if he wants to succeed in what is called in France *being something*, he has to give it up. This misunderstood expression does a lot of damage. In order to be *something*, a large part of the Nobility remains nothing.”⁶¹ The merchants felt that the intense practical training of business, the constant supervision and attention it required, and its remoteness from the leisure and finesse of the proprietary round of life kept them from cultivating the social and intellectual qualities that brought respect.⁶² Savary, whose *Le parfait négociant* remained throughout the eighteenth century a desktop oracle of business practice and morality, warned merchants not to educate their sons in the liberal arts and not to let them mingle with young nobles and men of the robe in the *collèges*, because the self-esteem they would acquire in those milieux would ruin them for trade.⁶³ Because these attitudes existed, anyone who remained in business, no matter how creditably he lived, suffered some discount in prestige. Even in the values of the Third Estate, diverse as they may have been, esteem was associated with proprietary wealth. Capitalism, which offered neither the assurance nor the standing that went with land and office, was simply a way, direct and dangerous, of getting rich.

⁵⁹ Quoted by Forster, *Nobility of Toulouse*, 118.

⁶⁰ Quoted by Warren C. Scoville, “The French Economy in 1700–1701: An Appraisal by the Deputies of Trade,” *Journal of Economic History*, XXII (No. 2, 1962), 247.

⁶¹ Gabriel-François Coyer, *La noblesse commerçante* (London, 1756), 205.

⁶² The debates of the National Assembly furnish a number of instances in which merchants apologized for their lack of verbal finesse, or were humiliated by oratorical failure, or even discussed the reasons why merchants were at a disadvantage in political argument. (See, e.g., *Archives parlementaires*, ed. Mavidal et al., IX, 94; XI, 70, 92–103; and P.-J. Nicodème, *Réflexions sur le commerce de toiles . . . adressées à l'Assemblée Nationale de France* [Paris, 1790]. The last is a pamphlet dated December 17, 1790: AN, AD⁴¹ 73.)

⁶³ Jacques Savary, *Le parfait négociant* (2 vols., Paris, 1777), I, 27–28.

The merchants, although they complained of the prejudices against trade, had to accept them as part of the status system and ground rules in the competition for standing. That is why they so often diverted profits into the purchase of country properties and offices, and why so many of them, once enriched, converted their commercial fortunes entirely into proprietary possessions. At an appropriate stage, the richest and most ambitious bought offices that conferred nobility. The members of the Danse family, linen merchants of Beauvais, constantly put business profits into country properties, acquired nobility, and, in 1757, liquidated their last partnership. During the Revolution, like other nobles and wealthy commoners, they lost their seigneurial dues, but purchased *biens nationaux* and remained until the Second Empire a family of provincial gentlemen.⁶⁴ But this is only a sample of what was going on. The conversion of commercial capital into proprietary wealth was a regular feature of French history, from the sixteenth century to the eighteenth and even beyond.⁶⁵ Apparently, the purpose of succeeding in business was to get the means of becoming a proprietor and a gentleman, and both Colbert and Necker, a century apart from one another, complained that this tendency drained off commercial and industrial capital and undermined economic growth.⁶⁶ In order to counteract this, the government frequently authorized nobles to enter maritime and wholesale commerce and banking, thereby permitting ennobled merchants to continue in trade without losing status.⁶⁷ This remedy, however, was only partly effective. It protected the

⁶⁴ Goubert, *Familles marchandes*, *passim*.

⁶⁵ For example, the Crozat, Toulouse merchants who passed, under Louis XIV, into finance and then became nobles and ultimately proprietors and military officers in Paris, allied by marriage with the *grande noblesse*. Also the Bruny of Marseilles, *armateurs* who passed into the *noblesse de robe*, represented at the outset of the Revolution by the Baron de la Tour d'Aigues and his cousin, the Marquis de Bruny d'Entrecasteaux, both *présidents à mortier*. Also the Couturier, Saige, and Lynch families of Bordeaux, Comte Wailsh of Nantes, and many others.

⁶⁶ Colbert: "Most of the money of the kingdom instead of being thrown back into business [that is] useful to the State, is used in the commerce of offices." (Quoted in Bertrand Gille, *Les origines de la grande industrie métallurgique en France* [Paris, 1947], 125.) Necker: "I do not hesitate to say that in France these provisions [the sale of offices] obstruct the whole development of the power & spirit of business; & that this is one of the principal causes of the superiority in several branches of commerce that those nations retain in which distinctions of status are less perceptible, & where all the pretensions that result from this do not constitute a constant matter of preoccupation." (Necker, *De l'administration des finances*, III, 149.)

⁶⁷ By laws of 1629, 1669, 1701, 1724, 1765, 1767, and 1787 nobles were authorized to participate in maritime commerce, banking, and wholesale trade. The titles and, occasionally, texts of some of these laws are given in *Recueil général des anciennes lois françaises depuis l'an 420 jusqu'à la Révolution de 1789*, ed. Athanase-Jean-Léger Jourdan *et al.* (29 vols., Paris, 1822-23), XVI, 339; XVIII, 217; XX, 400; XXII, 430, 470-71. The rationale of this legislation is discussed in two unpublished dissertations: Guy Richard, "La noblesse dans le commerce maritime au xviii^e siècle," *Mémoire pour la Diplôme d'Études Supérieures d'Histoire*, Université de Paris, 1952; Charles A. Foster, "Honoring Commerce and Industry in 18th Century France: A Case Study of Changes in Traditional Social Functions," doctoral dissertation, Harvard University, 1950. This legislation was intended to enable ennobled merchants to stay in trade without losing status and to encourage nobles of other backgrounds to enter business above the retail level. For the seventeenth century, it has been concluded, the legislation failed in both respects. (R. B. Grassby, "Social Status and Commercial Enterprise under Louis XIV," *Economic History Review*, XIII [No. 1, 1960], 19-38.)

juridical status of an ennobled merchant, but, since it had little impact on social values and attitudes, his sons were likely to drift into the administration, the armed forces, the judiciary, or country life, where sooner or later their ignoble origins would be forgotten.

There is no conclusive way of comparing the mass value of proprietary and business wealth in prerevolutionary France. Beginning with what passed in those days for statistics, supplementing them with estimates made by well-informed men who say little about their derivation, making inferences on assumptions which, though reasonable, can be endlessly debated, one concludes that the traditional modes of property—land, buildings, office, and *rentes*—accounted for more than 80 per cent of French private wealth.⁶⁸ This indicates a substantial preponderance for the proprietary sector. It is in no way astonishing. The day of heavy fixed industrial investment in factories and railroads, which would have altered the balance, lay far ahead. Meanwhile, most Frenchmen lived on the land, which yielded most of the taxable income and the gross national product. That is why the *économistes* not unreasonably attacked agricultural problems first, often to the neglect of the others.⁶⁹

⁶⁸ The starting point for the calculations leading to this conclusion is an estimate of national income for 1787, emanating from the Office of the Balance of Trade. (Ambroise-Marie Arnould, *De la balance du commerce et des relations commerciales extérieures de la France* . . . [2d ed., 3 vols., Paris, Year III (1795-96)], II, 263-71.) To explain how I have developed this estimate, and the sources from which I have taken supplementary data, would require an additional article, dense and closely reasoned. What appears below, then, is a statement of the results, which should be taken as provisional. The figures are in millions of livres tournois.

	Proprietary Capital	Commercial and Industrial Capital
French Agriculture	48,960	
Fisheries		1,300
West Indian Plantations and Slaves		2,000
Urban and Rural Buildings Rented	6,000	
Manufactures, Arts, and Crafts		505
Foreign Trade		3,745
Domestic Trade		6,977
Unspecified Commercial, Agricultural Capital and Omissions	580	580
Investments in Venal Office	1,500	
Bonds Posted by Officials	450	
Capital of Royal <i>rentes perpétuelles</i>	1,239	
Capital of Royal <i>rentes viagères</i>	1,237	
Outstanding Capital of Royal Lottery Loans	300	
Capital of <i>rentes perpétuelles</i> on Provincial Estates	172	
Investments in Private <i>rentes</i> and Loans, Unlisted Offices, Professional Practices, Uncultivated Land, Buildings Not Rented (Memorandum Only)	[?]	
	60,438	15,107

⁶⁹ For a provocative rehabilitation of the *économiste* Quesnay as the analyst par excellence of the old regime economy, see Lüthy, *Banque protestante*, II, 15-25.

For our purposes it is desirable to know the relative weight of the two kinds of capital not only for the society as a whole but in the upper Third Estate. Unfortunately, studies of the notarial records are not sufficiently advanced to show this. For the moment, all one can do is count persons, and from this it appears that even in the most heavily commercialized cities the proprietors and professional men in the Third Estate outnumbered the merchants. At Bordeaux, the second most active port, there were 1,100 officials, professionals, *rentiers*, and property owners against only 700 merchants, brokers, and sugar refiners.⁷⁰ At Rouen, a prime center of industry, banking, and maritime and wholesale trade, the administrative and judicial officers, professionals, and proprietors-*rentiers* outnumbered the merchants and brokers by more than three to one.⁷¹ At Toulouse, an agricultural, legal, and ecclesiastical capital, the ratio was about eleven to four, but the four included merchants who for the most part traded on small capital and in little volume and did much retail business, so that one hesitates to call them capitalists.⁷² There is, however, a further consideration. Because the merchants and industrialists owned, along with their commercial capital, considerable proprietary wealth, we could, with better data, divide them fractionally between the two sectors, and, by such a procedure, the share of commercial and industrial capital in the upper Third Estate would seem much lower than the impression we get by counting heads.⁷³

Soundings like these are merely straws in the wind, but they drift always in one direction. They confirm what seems to have been implicit in the consciousness of eighteenth-century France—that even in the well-to-do Third Estate proprietary wealth substantially outweighed commercial and industrial capital. This would not have surprised a Frenchman of the old regime and should not surprise us. The reason for stressing it here is to lay the ground for an assertion that is fundamental in analyzing the causes of the Revolution: there was, between most of the nobility and the proprietary sector of the middle classes, a continuity of investment forms and socioeconomic values that made them, economically, a single group. In the rela-

⁷⁰ "Tableau des corporations des arts et métiers de la Ville de Bordeaux et des classes qui composent le Tiers-État, ainsi que du nombre des individus dans chaque classe et corporation," MS prepared by the *armateur* Pierre-Paul Nairac and sent to Necker, Feb. 23, 1789, AN, B⁴ 22, *liasse* 38.

⁷¹ As against 687 royal administrative and judicial officers, professional practitioners, and *bourgeois non corporés*, there were 19 *armateurs*, 37 brokers, and 155 *négociants*. (*Cahiers de doléances du tiers état du bailliage de Rouen*, ed. Marc Bouloiseau [2 vols., Paris, 1957], I, lxxi-lxxii, 218-20.)

⁷² The four hundred merchants of the *Grand Tableau des Marchands* were outnumbered by two hundred officers of various jurisdictions, four hundred "bourgeois living nobly on their revenues," and five hundred members of the liberal professions. (Pierre-Henri Thore, *Essai de classification des catégories sociales à l'intérieur du tiers état de Toulouse* [offprint, Paris, 1954], 15-17.)

⁷³ See note 20, above.

tions of production they played a common role. The differentiation between them was not in any sense economic; it was juridical. This situation, in the historiography of the Revolution, has received practically no serious attention and remains, in Orwellian language, an "unfact." The reason for this is that it contributes nothing to what Cobban rightly calls "the established theory of the French Revolution," the theory that the Revolution was the triumph of capitalism over feudalism.⁷⁴ In that context the configuration of proprietary wealth that pervaded both the second and Third Estates has no place and remains unwanted, unused, and therefore, in effect, unknown.

It deserves, however, to be recognized, and its claims are strengthened by bringing forward a second unfact: that a substantial number of nobles participated as entrepreneurs in commerce, industry, and finance. There was indeed, before the Revolution, a *noblesse commerçante*, though not, perhaps, the one that the Abbé Coyer called for in 1756. Provincial, military, and court nobles, peers, and members of the royal family invested in the General Farm, speculated on the Bourse, and developed and exploited mines, canals, and metallurgical establishments, including the great foundry of Le Creusot.⁷⁵ On the other hand, there was, to reverse the phrase, a *commerce anobli*,⁷⁶ a sizable group of merchants ennobled through the municipal offices of certain cities and the two thousand or more venal offices that conferred nobility on the buyers.⁷⁷ For the most part, these ennobled merchant families were in

⁷⁴ Cobban, *Social Interpretation*, 8.

⁷⁵ Marcel Rouff, *Les mines de charbon en France au XVIII^e siècle* (Paris, 1922), and "Tubeuf, un grand industriel français au XVIII^e siècle," in *Mémoires et documents pour servir à l'histoire du commerce et de l'industrie en France*, ed. Julien Hayem, 7th Ser. (Paris, 1922), 1-126; Gille, *Origines de la grande industrie métallurgique*, and *Les forges françaises en 1772* (Paris, 1960), esp. 161; Pierre Léon, *Les techniques métallurgiques dauphinoises au dix-huitième siècle* (Paris, 1961), and *La naissance de la grande industrie en Dauphiné* (2 vols., Paris, 1953), I; Guy Richard, "La grande métallurgie en Haute-Normandie à la fin du XVIII^e siècle," *Annales de Normandie*, XII (No. 4, 1962), 263-89; XIII (No. 3, 1963), 165-76; P. M. J. Conturie, *Histoire de la fonderie nationale de Ruelle (1750-1940) et des anciennes fonderies des canons de fer de la Marine* (2 vols., Paris, 1951-52); *Les mines d'Anzin et d'Aniche pendant la Révolution*, ed. Alexandre-René de Saint-Léger (4 vols., Paris, 1935-39); *Histoire du Canal de Languedoc, rédigée . . . par les descendants de Pierre Paul Riquet de Bonrepos* (Paris, Year XIII [1804-1805]); Pierre Pinsseau, *Le Canal Henri IV, ou Canal de Briare (1604-1943)* (Orléans, 1943); Warren C. Scoville, *Capitalism and French Glassmaking, 1640-1789* (Berkeley and Los Angeles, 1950).

⁷⁶ This expression seems to have been invented during the discussion started by the Abbé Coyer, specifically by one P. Seras, who published at Brussels in 1756 a pamphlet entitled *Le commerce ennobli*.

⁷⁷ Bluche and Duryeu, *L'anoblissement par charges*, I, 23-38, corrects Henri Carré, *La noblesse de France et l'opinion publique au dix-huitième siècle* (Paris, 1920), 10, on ennoblement by municipal office. The mayoralities ennobled at Angers, Angoulême, Bourges, Nantes, Paris, and Poitiers; the *échevinage* (or *capitoulat*) at Lyons, Paris, and Toulouse. The mayor's lieutenants were ennobled at Angers and Poitiers. Ennoblement through the *échevinage* at Angers, Angoulême, Bourges, Niort, Poitiers, Tours, Nantes, La Rochelle, and Saint-Jean d'Angély was ended in the seventeenth century. The number of merchants ennobled by one means or another in the eighteenth century was considerable. In addition to the lists in the dissertations of Foster and Richard (see note 67, above), see Richard's four articles: "Les corporations et la noblesse commerçante en France au XVIII^e siècle," *L'information historique*, XIX (No. 5, 1957), 185-89, "La noblesse commerçante à Bordeaux et à Nantes au XVIII^e siècle,"

a transitional stage. As enterprises were liquidated, or generations arose that were no longer trained for business, they dropped out of trade to live, as other nobles did, on their revenues. All the same, merchants or not, they were nobles and sat in the noble assemblies of 1789. To sum up, there were nobles who were capitalists. There were merchants who were nobles. As the proprietary wealth traditionally identified with aristocracy extended far down into the Third Estate, so the capitalism traditionally identified with the wealthy Third Estate penetrated into the second, and into its highest ranks.

This means that the old diagram by which we envision prerevolutionary society must be changed. There was a clear juridical boundary that separated nobles from commoners, and a commoner crossed it by registering a legal document, his letters of nobility. On the other hand, the frontier between capitalist and proprietary wealth ran vertically through both orders. The horizontal line marked a legal dichotomy, the vertical line, an economic one. To think of them as coinciding, even roughly, is to misunderstand the situation completely. The concept of two classes, at once economically and juridically disjunct, can be sustained only by ignoring the weight of proprietary wealth in the Third Estate and that of capitalism in the second, or, in other words, by continuing to ostracize them as unfacts.

From this follow two important conclusions. The first is that when the word bourgeois is used to indicate a nonnoble group playing a capitalist role in the relations of production it includes less than half the well-to-do Third Estate and excludes the proprietary groups that furnished 87 per cent of the Third Estate deputation to the Estates-General.⁷⁸ In other words, it

ibid., XX (No. 5, 1958), 185-90, "À propos de la noblesse commerçante de Lyon au XVIII^e siècle," *ibid.*, XXI (No. 4, 1959), 156-61, and "La noblesse de France et les sociétés par actions à la fin du XVIII^e siècle," *Revue d'histoire économique et sociale*, XLI (No. 4, 1962), 484-523. The rationale and significance of all this are discussed in Henri Lévy-Bruhl, "La noblesse de France et le commerce à la fin de l'ancien régime," *Revue d'histoire moderne*, VIII (No. 3, 1933), 209-35; and Marcel Reinhard, "Élite et noblesse dans la seconde moitié du XVIII^e siècle," *Revue d'histoire moderne et contemporaine*, III (Jan.-Mar. 1956), 5-37.

⁷⁸ Merchants, bankers, and manufacturers constituted only 13 per cent of the Third Estate deputies in 1789. (Cobban, *Myth of the French Revolution*, 23-25.) Also, more than half these entrepreneurs lived in the country (some were forgemasters or merchants of wood or horses) or in small towns and are poorly described by the word "capitalist." Early in 1791 there was published a list of deputies who were detested for their extremism. But this political group was sociologically diverse, as the percentages show. A fourth were members of the formerly privileged orders, 11 per cent being nobles and 14 per cent clergy. Only 9 per cent were in business. The remaining two-thirds were officials, professional men, and landowners. (R. R. Palmer, "Sur la composition sociale de la Gauche à la Constituante," *Annales historiques de la Révolution française*, XXXI [No. 2, 1959], 154-56.) Although of all professional groups the men of law played the most prominent role in the Third Estate of 1789, they have received little study. The professional background of their political activity is explained in a valuable article by Philip Dawson, "The bourgeoisie de robe in 1789," *French Historical Studies*, IV (No. 1, 1965), 1-21.

embraces only a minority of the upper middle classes and explains almost nothing about the origins of the revolutionary leadership. In this sense it should be discarded as inadequate and misleading. But there are other senses, loaded with eighteenth-century implications, in which the word will continue to be employed because it alone translates what the documents have to say. One may, for example, speak of bourgeois who lived nobly on their revenues and comprised a fiscal category; these constituted a small portion of the Third Estate and counted entirely in the proprietary group. One may also speak of bourgeois as persons who, being inscribed in the registers of the bourgeoisie of a town, enjoyed what Anglo-Saxons call "the rights of the city," including political advantages and fiscal exemptions worth having, but in this sense the bourgeoisie included nobles and noncapitalist commoners and was not entirely of the Third Estate.⁷⁹ Finally, one may adopt a peasant usage, applying the word bourgeois to townsmen who collected rents in and near the village and were felt to be an alien and adverse interest.⁸⁰ All three meanings convey realities of the old regime and are useful on condition that one makes clear which of them he has in mind.

The second conclusion is that we have no economic explanation for the so-called "bourgeois revolution," the assault of the upper Third Estate on absolutism and aristocracy. No one denies that such an assault took place or that it left a powerful imprint upon French society. The struggle for the doubling of the Third Estate and the vote by head, the demand for a constitution and an elected legislature, the intimation of political equality in the Declaration of the Rights of Man, the liquidation of intendancies, provinces, parlements, fiscal inequalities, forms of nobility—all these, put in series with the emigration, the expropriation of Church and *émigré* wealth, and the Terror, have to be made credible on some basis. By one of the unexamined postulates of current historiography we expect them to be explained by a conflict of social classes and the contradictions between a "rising" economic order and the order that it challenges. The position taken here is that we have now learned enough to see that this cannot be done, that to divide the wealthy elements of prerevolutionary society into a feudal aristocracy and a capitalist bourgeoisie forces the concealment of too much evidence, and that the whole classic concept of a bourgeois revolution has become impossible to sustain.

This leaves in our interpretation of the Revolution a somewhat painful void. Our instinct is to fill it with a new class struggle interpretation like

⁷⁹ Contemporary definition and discussion in *Encyclopédie méthodique: Jurisprudence*, II, 96–98; X, 442–45.

⁸⁰ Paul Bois, *Paysans de l'Ouest: Des structures économiques et sociales aux options politiques depuis l'époque révolutionnaire dans la Sarthe* (Le Mans, 1960), 340–43.

Cobban's "revolution of the propertied classes,"⁸¹ which explains some results of the Revolution but not, apparently, its origins. There may, however, be more plausibility in a political approach than in a reorganization of social categories. The gist of such an approach can be set down in two propositions that probably amount to the same thing. First, the struggle against absolutism and aristocracy was the product of a financial and political crisis that it did not create. Second, it was essentially a political revolution with social consequences and not a social revolution with political consequences. Because these assumptions suggest a backward step in historiography, it will take a few paragraphs to make them respectable.

The Revolution resulted from a bankruptcy that left the monarchy discredited and helpless. The disclosures of the first Assembly of Notables shocked everyone capable of reacting to public affairs, set off an expanding discussion of reforms, and raised hopes for a national regeneration.⁸² The government's reform program, which threatened privileges and seemed tainted with the supposed negligence and dishonesty of the Controller General Calonne, was rejected by the Notables. For more than a year the parlements and other constituted bodies opposed it. This resistance, the so-called *révolte nobiliaire*, taught the upper Third Estate the language, tactics, and gallantry of opposition. It made the convocation of the Estates-General inevitable. When in August 1788 this convocation was announced (along with a partial suspension of payments), there was thrust upon the nation a new political issue: whether royal power would pass to the privileged orders or would be shared with those who, until then, had been disfranchised. By inviting his subjects to advise him on how to organize the Estates-General, the King precipitated a landslide of publications that touched off a growing outcry for the doubling of the Third and the vote by head. This generated a political struggle between democracy, as Palmer has defined it, and aristocracy, substantially as he has taught us to understand it. The stakes were very high. They included the question of at whose expense the financial problem would be solved, and whether careers in the military, the clergy, and the judiciary, and, above all, in politics would be opened to commoners, rich and poor, whose main resources were talents, education, and ambition. In explaining the democratic assault on despotism and aristocracy it is unnecessary to conjure up a social struggle rooted in economic change. The paralysis of the monarchy, the apprehensions of the taxable groups and creditors of the state, and the hopes and ambitions of the professional classes, combined with the slogans, myths, and images generated by the struggle, seem quite enough

⁸¹ Cobban, *Social Interpretation*, Chap. xii.

⁸² Egret, *Pré-révolution*, 369.

The revolutionary mentality was created by the crisis. It was, in fact, the writing of the *cahiers* that forced a crystallization of issues and their formulation in ideological terms. For the mass of the upper Third Estate, the schools of revolution were the electoral assemblies of 1789, not the salons and *sociétés de pensée* of the old regime.

What this interpretation restores is the sense of an unplanned, unpremeditated revolution that in many ways exceeded the aims expressed in the *cahiers de doléances* of March and April 1789. Take, for example, the abolition of nobility, which may be understood here as aristocracy constituted juridically as an order. If in the spring of 1789 the upper Third Estate had seen nobility as an intolerable institution it would certainly have called for its destruction. But this was never attempted until the revolutionary leadership had concluded, from more than a year of political experience, that the nobility was an incorrigible enemy of the new regime. Certainly there was friction in the quarrel of 1788 over how the new provincial estates would be constituted and whether nobles and commoners would deliberate there together. It was intensified by the dispute over how the Estates-General should be organized. But in the spring of 1789 middle-class feelings toward nobility were still benign. Far from wanting to abolish nobility, the Third Estate wished to rehabilitate it. One reads in the Third Estate *cahiers* of the major towns and cities that nobility was to be reformed, that nobles should be given opportunities to replenish their fortunes, and, still more remarkable, that nobility must be saved from adulteration by abolishing the venal offices and making ennoblement depend not on money but on service to the nation.⁸³ Then came the quarrels and confrontations of 1789, the destruction of the constituted bodies, and the reform of the army and the Church, which was dispossessed to protect the creditors of the state. These events made the opposition to the Revolution, inside and outside the National Assembly, formidable. In all three orders it developed considerable strength. On June 19, 1790, after a year of struggle, nobility, as such, was abolished in order to dis-

⁸³ In the Third Estate *cahiers* I have yet to find any demand for abolishing nobility as an institution. The following are *cahiers* whose authors either denounced venal ennoblement or called for its elimination: Third Estate, province of Poitou, *bailliage* of Tours, *bailliage* and *sénéchaussée* of Poitiers, *bailliage* of Amiens, town of Châlons-sur-Marne, *sénéchaussée* of Montauban, city of Rouen, *sénéchaussée* of La Rochelle, city of Nantes, city of Paris (Intramuros), city of Marseilles, city of Orléans, town of Angoulême. (*Archives parlementaires*, ed. Mavidal et al., I, 746; II, 597-98; III, 482; IV, 99; V, 410, 416, 493, 601-602; VI, 53; *Les élections et les cahiers de Paris en 1789*, ed. C.-L. Chassin [4 vols., Paris, 1888], III, 337; *Cahiers de doléances de la sénéchaussée de Marseille pour les États généraux de 1789*, ed. Joseph Fournier [Marseilles, 1908], 367; *Cahiers de doléances du bailliage d'Orléans pour les États généraux de 1789*, ed. Camille Bloch [2 vols., Orléans, 1906-1907], II, 323; *Cahiers de doléances de la sénéchaussée d'Angoulême et du siège royal de Cognac pour les États généraux de 1789*, ed. Prosper Boissonade [Paris, 1907], 103.) For an explanation of the paradox of why the *roturiers* should have desired closure of the road to ennoblement through venal office, see Elinor G. Barber, *The Bourgeoisie in 18th Century France* (Princeton, N. J., 1955), 56-57.

arm and probably to punish the most conspicuous element of the opposition.⁸⁴ Nothing in the *cahiers* forecasts such a decree. The intention to smash the legal basis of nobility and, along with it, the whole system of language, symbols, images, and formalities that reinforced the subservience of the lower groups, was a product of the revolutionary crisis, not a cause. To argue that it came about through long years of economic change, class formation, and the gradual growth of class consciousness in a bourgeoisie that played a capitalist role in the relations of production is not only out of keeping with the evidence, but superfluous.

The present crisis in the interpretation of the French Revolution results from the maturing of social history as a discipline. This specialty, in its present form, was virtually created in France.⁸⁵ Its methods are as distinctive as the sources it employs, and its findings are most convincing when expressed in quantitative form. Applied to the history of the Revolution, it has yielded a mass of data on economic interests and conditions, standards of living, population change, corporate structures, social values, and the complex mentalities found at various levels of society. Much of this material disagrees with the vocabulary in use when the effort began. But the vocabulary is still in force. The problem is how to rescue the data from a language that misrepresents it and imprisons it in categories that can no longer be justified.

Although interest in the social history of the Revolution is very old, its progress as a specialty began during 1901-1904, when Jaurès published the first four volumes of the *Histoire socialiste*⁸⁶ and procured the establishment of the Commission of the Economic History of the Revolution.⁸⁷ "It was Jaurès," Lefebvre once wrote, "who habituated historians to see [in the Revolution] a fact [that is] social and, consequently, of economic origin."⁸⁸ Jaurès had no doubt that the Revolution was the political triumph of a bourgeoisie matured by the growth of capitalism, and, with an erudition that is astonishing, given the literature available to him, he rewrote the history of the Revolution on this theme. Lefebvre, who avowed a deep in-

⁸⁴ *Archives parlementaires*, ed. Mavidal et al., XVIII, 104-10.

⁸⁵ Although social history was first systematically defined by Lucien Febvre and Marc Bloch, the first great monograph in the field, still an authoritative model, was Georges Lefebvre's *Les paysans du Nord pendant la Révolution française* (Lille, 1924; 2d ed., Bari, 1959).

⁸⁶ *Histoire socialiste*, ed. Jean Jaurès (12 vols., Paris, 1901-1908), I-IV. These are Jaurès' four volumes on the Constituante, the Legislative, and the Convention, published during 1901-1904 and republished twenty years later by Albert Mathiez under the title, *Histoire socialiste de la Révolution française* (8 vols., Paris, 1922-24).

⁸⁷ Since its establishment in 1903, the commission and its affiliated committees have published well over two hundred volumes of documents, papers, monographs, and research guides. Many of these are indispensable *instruments de travail*.

⁸⁸ Lefebvre, "La Révolution française et les paysans," *Études*, ed. Soboul, 339.

debtedness to Jaurès,⁸⁹ never renounced this view. In the first two paragraphs of *Quatre-vingt-neuf*, paragraphs that dominate the reading of the whole book, he identified the "primary cause" of the Revolution as a conflict between an aristocratic society, grounded historically in the ascendancy of land-owners, and a new class, the bourgeoisie, enriched on liquid forms of wealth. In this passage Lefebvre left no doubt that capitalism was the economic basis of the bourgeoisie and the source of its growing power. Out of this socio-economic configuration had come, he said, the ideology of the philosophes and the *économistes*, expressing the values and aspirations of a revolutionary class. These developments were fundamental. The royal bankruptcy and the aristocratic resistance that forced the King to convoke the Estates-General were treated as an "immediate cause" which explained many of the characteristics of the Revolution and why it began when it did.⁹⁰

Lefebvre's work, however, led him to modify considerably the original overview of Jaurès. Writing in 1932, he found that overview already too simple. As an explanation, he observed, it was credible only when supplemented with the financial crisis, the *révolte nobiliaire*, and the economic distress that produced the popular disturbances without which the Revolution could not have succeeded.⁹¹ In *Quatre-vingt-neuf*, passing well beyond the thesis announced in the preface, he described four revolutions: aristocratic, bourgeois, popular, and peasant.⁹² In *La Révolution française*, the synthesis that he contributed in 1951 to the series "Peuples et civilisations," he described an aristocratic revolution, a bourgeois revolution, and a popular revolution, the last being composed of a Parisian revolution, a municipal revolution, and a peasant revolution; all these were treated under the heading "L'avènement de la bourgeoisie en France."⁹³ He was also troubled, far more

⁸⁹ "It is really Jaurès who has been our master and the more worthy of being so in that, while restoring to history its social and economic substructure, he corrected what was intemperate in what went by the name of Marxism, by maintaining very energetically that ideas yet have a life of their own, that they play an essential role in history, that the dominant class itself is not wholly governed by egoist preoccupations and that there is sincerity in its conviction that the general well-being depends on the maintenance of its authority." (*Ibid.*; see also Soboul, "Georges Lefebvre historien de la Révolution française [1874-1959]," *ibid.*, 3-4.)

⁹⁰ Georges Lefebvre, *Quatre-vingt-neuf* (Paris, 1939), 5-6; *id.* *The Coming of the French Revolution*, tr. R. R. Palmer (Princeton, N. J., 1947), 1-2. Also a passage of 1937: "The origins of the Revolution involve several problems. It was the bourgeoisie that took control of it and gave it juridical form, inspired by its ideology, which agreed with its interests. The problem—which Jaurès stated clearly and for which he outlined the solution—is to know how the bourgeoisie finally came to understand, thanks to the progress of the capitalist economy, that feudal institutions opposed to the triumph of capitalism an obstacle that it was absolutely necessary to eliminate; and how at the same time it acquired the strength, the knowledge, and the talent, which, with the consciousness of its social superiority, gave it the will and the means to seize power and keep it." (Lefebvre, "Le mouvement des prix et les origines de la Révolution française," *Études*, ed. Soboul, 233-34.)

⁹¹ *Ibid.*, 340-41.

⁹² Lefebvre, *Quatre-vingt-neuf*, 9-172.

⁹³ *Id.*, *La Révolution française* (Paris, 1951), 107-88.

than less perceptive historians, by the problem of relating the bourgeoisie, with all its diversity, to the derivation assigned it in Jaurès' writings and his own preface to *Quatre-vingt-neuf*. Twice he wrote that it was not "homogeneous."⁹⁴ In *La Révolution française* he saw it as composed of bourgeois living on investments in land and, to some extent, liquid capital; holders of venal offices; financiers, maritime merchants, and manufacturers; a "middle class" or *petite bourgeoisie* of tradesmen and petty officials; and a bourgeoisie of intellectual capacities ranging from savants and artists to law clerks and office employees.⁹⁵ The determinants of status, he believed, included birth, corps, vocation, and, occasionally, talent. In his last study, an analysis of the urban society of Orléans, he laid out social categories in terms of order, vocation, and wealth or income,⁹⁶ but Soboul tells us that he was not satisfied with either the method or the results.⁹⁷ It is not difficult to see why. Classification by wealth conflicted with classification by role, and both conflicted with classification by order. Nearly a fifth of the nobles who enjoyed revenues of more than five hundred livres per year, for example, were merchants and sugar refiners; "bourgeois" by vocation, they shared the privileges of the second estate. To put the matter another way, half the refiners and a third of the merchants named in the tax rolls of 1791 were nobles; giving priority to the system of orders, Lefebvre classified them with the nobility. The Third Estate he divided into a *haute bourgeoisie* and a large category called *moyenne et petite bourgeoisies*, but for lack of tax rolls did this entirely on the basis of vocations and corporate groupings. All nonnoble merchants, refiners, brokers, officials, and manufacturers were assigned to the *haute bourgeoisie*, although Lefebvre observed that, if the tax rolls had survived, some of them would have had to be demoted. On that principle, of course, the same documents would have elevated many professional men from the lower group to the higher. Finally, one reads that the *cahier* of the Third Estate of the *bailliage* was drawn up by the elite of the bourgeoisie, but that elite, a political entity, remains unreconciled with the socioeconomic groupings.⁹⁸

Apparently, what the emerging data have made impossible is to equate the identifiable leadership of the upper Third Estate—the "revolutionary bourgeoisie"—with a social class that played a common role in the relations of production, or, more precisely, owned the instruments of production in an emergent capitalist economy. Soboul, in his masterful study of the sans-

⁹⁴ *Id.*, *Quatre-vingt-neuf*, 46, and *La Révolution française*, 49.

⁹⁵ *Ibid.*, 48–52.

⁹⁶ Lefebvre, *Études orléanaises*, I, 137–209.

⁹⁷ Soboul, "Georges Lefebvre," 17.

⁹⁸ Lefebvre, *Études orléanaises*, I, 209.

culottes, faced a comparable situation. He found the sans-culottes a political bloc composed of diverse economic elements; he therefore pronounced them not a social class.⁹⁹ The same step may now be taken with regard to the "revolutionary bourgeoisie." Jeffry Kaplow has, in fact, moved toward this solution by defining the bourgeois on juridical and political lines.¹⁰⁰ They were, he says, well-to-do people excluded from the privileges of the nobles and from powerful positions in the state, the army, the Church, and the parlements. Yet they had access, not enjoyed by the common people, to local political office. "They were beginning to become conscious of themselves as a class," he observes, "and shared a definite set of values." That is certainly true. Yet, if this is a social class, it is not one in the sense recognized by the last two generations of social scientists in this country. Nor is it the bourgeoisie as we commonly think of it.

Hexter has recently pointed out that one of the peculiarities of historical rhetoric is the use of words that he calls "evocative" because they signal the historian to summon up whole categories and sequences of associations with which professional thought identifies them.¹⁰¹ Terms like "aristocracy," "bourgeoisie," "feudalism," "capitalism," and "social class" have this quality. It is what gives them interpretive value. Each is freighted with implications that make it operative in the machinery of the bourgeois revolution model, so that, as Cobban points out, to accept the language is to accept the theory. In ordinary usage, whoever says "class" is heard to say "productive role," and whoever says "bourgeois" is heard to say "capitalist." Unless he adds an emphatic disclaimer, he should expect to be understood in this sense. But even emphatic disclaimers can be ineffectual if, as in the case of "class" and "bourgeois," special meanings have been welded on by more than thirty years of writing, teaching, and discussion. Under those circumstances, there is little prospect of revising professional usage. That is particularly true of a vocabulary which, among many millions of the world's people, has a content that is ideologically obligatory and is thereby frozen into alliance with an obsolete interpretation. Obviously, the project of solving this problem by giving new meanings to old words is more or less utopian. The phrases "bourgeois revolution" and "revolutionary bourgeoisie," with their inherent deceptions, will have to go, and others must be found that convey with precision and veracity the realities of social history.

⁹⁹ Albert Soboul, *Les sans-culottes parisiens en l'an II: Mouvement populaire et gouvernement révolutionnaire, 2 juin 1793-9 thermidor an II* (Paris, 1958), 427.

¹⁰⁰ In his introduction to *New Perspectives on the French Revolution: Readings in Historical Sociology*, ed. Jeffry Kaplow (New York, 1965), 14.

¹⁰¹ J. H. Hexter, "The Rhetoric of History," a paper read at the Annual Meeting of the American Historical Association, San Francisco, Dec. 28, 1965, to be published shortly in *History and Theory*.

Class in the French Revolution: A Discussion

I

On "Who Intervened in 1788?"

JEFFRY KAPLOW*

IN her review article, "Who Intervened in 1788? A Commentary on *The Coming of the French Revolution*" Professor Eisenstein sought to show that Georges Lefebvre's claim that it was the bourgeoisie who initiated revolutionary action against the parlements in September 1788 is not supported by the evidence.¹ This revisionist view calls for several comments.

First, it should be noted that Eisenstein is really a new kind of revisionist. Unlike some of her predecessors, she refuses to throw out the concept of the bourgeoisie, although she maintains that it is a "difficult to define social sector." Her own view is much more radical—that the existence of a bourgeoisie, however defined, is irrelevant to the discussion, for the theory of class from which the concept proceeds is

a static framework derived from a structural analysis . . . incapable of containing this sort of dynamic group action [that is, the action of the national party in the fall and winter of 1788]. Instead it keeps apart, as socially stratified, the very cluster of men who gravitated together, mutually attracted by political goals that appeared to be within their reach. By artificially segmenting this continuous group action, by arbitrarily assigning revolutionary initiative first to the class-oriented activities of the aristocracy, then to those of the bourgeoisie, the author [Lefebvre] has, thus, almost smothered his evidence.²

Now this argument, ingenious though it may be, sins in two ways. First, it sets up a theoretical straw man, and, second, it neglects to make use of evidence gathered by scholars since Lefebvre wrote in 1939.

Any theory of class analysis that proposes a static model is fundamentally ahistorical and therefore inapplicable to the matter at hand. Lefebvre and his successors—George Rudé, Albert Soboul, Richard Cobb, and others—many of whom approach the history of the French Revolution from a Marxist or Neo-Marxist standpoint, are aware of this and are at pains to stress the con-

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¹ Elizabeth L. Eisenstein, "Who Intervened in 1788? A Commentary on *The Coming of the French Revolution*," *American Historical Review*, LXXI (Oct. 1965), 77-103, esp. 79.

² *Ibid.*, 102.

stant evolution of classes, their inner complexity, and the interaction that takes place between them and their social environment. They would, I think, agree with Eisenstein that there was no clear-cut dichotomy between any two large classes in old regime France. I suggest that their use of words like "nobility" and "bourgeoisie" does not in any way imply a belief in the homogeneous nature of the two groups or in their political solidarity, as Eisenstein seems to infer.³ Certainly, the distinction between noble and bourgeois in the eighteenth century was not so absolute as that between capitalist and proletarian in the nineteenth, for instance. And the internal cohesion of each class has also gained much in the telling. There were nobles of the robe and nobles of the sword, those of the court and those who resided in the provinces, the rich and the poor, the officeholders and those deprived of participation in government. The members of the bourgeoisie also differed from one another according to criteria of profession, wealth, residence, and status, to name only a few. Furthermore, there were surely nobles who played bourgeois roles (by holding capitalist investments, for instance) and bourgeois who became noble, but did not give up the activities nor the attitude of mind that made them bourgeois. In the light of this, is there any meaningful sense in which the bourgeoisie and the nobility can be said to have been fundamentally opposed to one another? Indeed, there is. Each represented a different stage in a complex set of socioeconomic relationships, the one feudal, the other capitalist. The dichotomy is not between the purely feudal relationships of Charlemagne's time and the industrial capitalism of the nineteenth century, both of which terms are historically anachronistic when applied to our period. But the conflict remains, and associated with it is a set of political choices. Contrary to what has been said and repeated a thousand times, these choices were not centered around the narrow conflict between aristocracy and democracy, still less around a particular set of political institutions, but rather around the questions of who should rule and, more important still, who should have access to the levers of power. The choice between Parlement and Parliament was less important than that between the stasis of a noble-oriented society based on birth, privilege, and honor, on the one hand, and the dynamic society of the bourgeoisie with its watchwords of talent, intelligence, and productivity, on the other.

Now it would be a grave error to assume that belonging to a given class automatically determines one's political attitudes. This sort of crude determinism has long been out of fashion with historians of the French Revolution, and the use of it constitutes another instance of Eisenstein's straw

³ *Ibid.*, 85.

man technique. To expect the nobility to act in defense of its interests (as it did, for the most part) is one thing; to hold that each noble will respond to a given stimulus like Pavlov's dogs is quite another. While the first expectation is borne out by the facts, the second is shown to be indefensibly mechanical.

The author's argument falls into two main parts: first, that "France's bourgeoisie" did *not* initiate the protest movement of 1788 and did *not* play a prominent role in the events and reforms of 1789; second, that this leading role, usually attributed to the bourgeoisie, was in fact played by a heterogeneous group of men "mutually attracted by political goals that appeared to be within their reach."⁴ On the first point, she holds that there is no evidence to prove that "protest against the Parlement's ruling [of September 25, 1788] was locally initiated by groups drawn exclusively from any one class or estate."⁵ That is true enough if the emphasis is placed on the word *exclusive*. The fact is that the bourgeoisie did intervene in numerous provincial cities.⁶ Although they were not alone, they did constitute the new element in the struggle, not having previously played any but a passive role. Every bourgeois did not participate in the movement equally, and some—like the representatives of privileged towns in Brittany or some guild members in Dijon—even opposed it. For that reason, they would soon be removed from positions of authority and replaced by bourgeois of a politically more radical sort. For someone who insists that it is indeed the struggle over representation in the Estates-General that distinguishes the Revolution from all preceding "times of troubles," Eisenstein is curiously blind to the agitation led by these men. What is surprising is not that some bourgeois remained outside of politics but that so many did in fact rally to the national cause. As Michel-Joseph-Antoine Servan, the former *avocat général* of the parlement of Grenoble, put it:

Ils [le Tiers État] n'attendent de fortune que de leurs services et de distinction que de leurs bassesses auprès de la Noblesse et du Clergé. Les bénéfices d'un côté; les offices de judicature, de l'autre. Tout ce que les dons ont de réel, tout ce que les promesses et les illusions de l'espérance ont de séduisant; que de chaînes dans les mains de la Noblesse et du Clergé pour accabler le Tiers État, qui les reçoit en les baisant, tantôt comme honorables, tantôt comme sacrées! Que de moyens enfin d'enlever à la partie du Tiers État qui s'ignore elle-même, celle qui serait capable de connaître ses droits et de les défendre!⁷

⁴ *Ibid.*, 101, 102.

⁵ *Ibid.*, 83.

⁶ See the work of Jean Egret, in particular his "Les origines de la Révolution en Bretagne (1788-1789)," *Revue historique*, CCXIII (Apr.-June 1955), 189-215, tr. and reprinted in *New Perspectives on the French Revolution: Readings in Historical Sociology*, ed. Jeffrey Kaplow (New York, 1965). It seems to me odd, to say the least, that Eisenstein makes no mention of Egret's work, for he is by common consent the outstanding authority on the period under discussion.

⁷ Jean Egret, *La pré-révolution française (1787-1788)* (Paris, 1962), 351-52.

The chains of subordination are hard to break. The bourgeoisie, like many another oppressed class since that time, had internalized the portrait drawn of it by the establishment, and that is a situation less conducive to revolution than to Uncle Tomism.

Because this was true, the liberal nobility in the Committee of Thirty rendered the bourgeoisie a great service by making propaganda in favor of the good cause. Why did they do so? We cannot say without undertaking biographical study of the individuals involved. That many of the Thirty were members of the robe engaged in a struggle with the monarchy may be one reason. Mirabeau's desire to strike a blow against the society that had effectively disowned him may be another. Adherence to political goals or the charisma of leadership may have to be taken into account. In any case, the constant reminder that certain aristocrats did in fact go so far as to support the doubling of the Third proves nothing whatsoever about the class nature of the struggle of 1788. Nor will it do to kick in an open door by arguing, as Eisenstein does, that the clergy was in fact not a class.⁸ Mallet du Pan was a more accurate journalist than the author thinks when he wrote, in January 1789, that "The war is between the Third Estate and the other two orders."⁹ What he—and, I would argue, most of his contemporaries—meant when they spoke of the clergy was that portion of it made up of great abbots and bishops, but certainly not the parish priests. In the Third Republic, a priest was a priest, and that was that, but that was not the case in the old regime.

Let us now look more closely at the specific issue at hand: the doubling of the Third. A significant section of the bourgeoisie wanted this change, as did some aristocrats. It will not, however, do to play with the evidence in order to show that there were no essential differences between the attitudes of the two classes. Thus, it is true that the First Bureau of the Assembly of Notables of 1788 voted, under the leadership of the Comte de Provence, for the doubling, but Eisenstein neglects to quote the second half of Lefebvre's sentence to the effect that this was done "on condition that each order in the Estates General should remain free to accept or reject the vote by head"—a provision that effectively took the guts out of the resolution.¹⁰ Furthermore, she argues, without a shred of evidence, that the patriot leaders used old electoral traditions to keep the issues of the doubling of the Third and the mixing of the orders separate until after the elections, so that the Third Estate would be free of representatives of the clergy and nobility, who

⁸ Eisenstein, "Who Intervened in 1788?" 82.

⁹ *Ibid.*, 85.

¹⁰ *Ibid.*, 82; see also Georges Lefebvre, *The Coming of the French Revolution*, tr. R. R. Palmer (Princeton, N. J., 1947), 59, and Egret, *Pré-révolution*, 344.

normally would have been elected in the place of the "literate laity."¹¹ Even if this were true—and it presupposes a centrally organized electoral campaign whose existence is not proven—it would say nothing about the vote by head. Mixing one estate with another for the purposes of debate does not automatically imply voting as individuals. If we are to believe Mirabeau, it may well be that some parlement members of the Committee of Thirty were not just using a tactic in desiring to keep the issues separate, but were actually expressing a preference: "yes" to the doubling of the Third, but no farther than that on the democratic road.¹²

There are a couple of other points that have to do with the use of evidence. Eisenstein accuses Lefebvre of contradicting himself on two occasions. The first concerns the third order of the provincial estates of Brittany. The Third Estate was made up of privileged persons, but they nonetheless defied the first and second estates by refusing to sit until fiscal equality was granted. How can one explain the apparent paradox? If we consult Jean Egret, we find that the persons in question, privileged though they were, acted daringly, precisely because of the presence in the meeting room of some twenty-nine municipal deputies and several *commissaires adjoints* from Nantes, who represented the revolutionary force of the Breton bourgeoisie.¹³ Another example: Lefebvre is guilty of contradiction in arguing that the central organization of a revolutionary movement was inhibited by uncertain means of communication, while at the same time stating that news of the Parlement's September ruling spread quickly. To this it must be said that the spreading of news and the organizing of revolution cannot be lumped together when discussing the problem of communications. To spread the news of a parlement's action was perfectly legal and above board; to organize a revolution is to engage in clandestine activity. Newsboys and revolutionaries have not the same access to transportation facilities.¹⁴

The Third Estate was indeed represented in the Estates-General by members of the literate laity—who also happened to be bourgeois. I would be the last to say that our concepts of class in preindustrial societies could not stand sharpening and, perhaps, redefinition. But it is a quibble to take Lefebvre to task for his use of the expression "enterprising bourgeois" when speaking of those responsible for drawing up the *cahiers* and suggesting candidates. Most of them were not entrepreneurs, but they were enterprising.

¹¹ Eisenstein, "Who Intervened in 1788?" 95, 96–97.

¹² Egret, *Pré-révolution*, 328.

¹³ Eisenstein, "Who Intervened in 1788?" 83; Egret, "Bretagne," 203; see also Lefebvre, *Coming of the French Revolution*, 18–19, 60–61.

¹⁴ Eisenstein, "Who Intervened in 1788?" 94.

In my view, it is impossible to discuss the making of the Revolution solely in terms of who was responsible for a given set of actions at its beginning, without making reference to the program and accomplishments of the Revolution as a whole. But even if we permit Eisenstein her approach, it must be said that she has failed to make her point. When she says that "There is something wildly askew about a structural model that includes the top layer of nobles and the bottom layer of the clergy within the middle ranks of the 'middle' class,"¹⁵ she is accusing only herself. She has lumped these people together on criteria of political ideas and literacy. No one else has ever done so—at least not on the basis of a social definition. What Lefebvre and his followers have undertaken to prove is that the French Revolution was a bourgeois one, in that it was made by bourgeois for the benefit of the bourgeoisie—or, if you will, in the name of an ideal formulated by the bourgeoisie and identified with the well-being of humanity as a whole. The facts show that the bourgeoisie was active, that it had ideals, although its members differed among themselves on specific issues within the general framework and, of course, on the question of means. They were not alone, but they were dominant. All our questions have not been answered, but a foundation has been laid. Is it not possible, at long last, to go on from there?

¹⁵ *Ibid.*, 94-95, n. 27.

II

The Many Lives of Georges Lefebvre

GILBERT SHAPIRO*

DURING the past few decades, studies of the French Revolution have flowered. A wide consensus seems to have been developing, muffling the traditional conflicts of rival ideological schools in a mass of documentary evidence. Empirical studies of theoretically crucial issues were pursued by new techniques, particularly in economic history and in the study of the social origins of participants in various revolutionary groups and activities. In the wake of such a wave of progress it is, I suppose, both healthy and inevitable that we should witness a wave of skepticism. Elizabeth Eisen-

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stein's review article on Lefebvre's *The Coming of the French Revolution*¹ is best understood as one instance of this reaction, along with Alfred Cobban's important volume, *The Social Interpretation of the French Revolution*,² Richard Cobb's wide-ranging attacks on new methods in history, at the 1966 meeting of the Society for French Historical Studies and in the *Times Literary Supplement*, and George Taylor's brilliant contribution to the session on the French Revolution at the American Historical Association meeting in San Francisco and published in this same issue of the *American Historical Review*.

Despite some serious differences in their approaches, Eisenstein's main purpose is, like Cobban's, to show how a traditional Marxist approach to the French Revolution is contradicted by the growing body of evidence (which, paradoxically, has been largely collected by "Marxists"). Outside of China and Albania, it is difficult to imagine anyone objecting to this ambition. As "Neo-Marxists," whether sociological theorists (such as Ralf Dahrendorf or Norman Birnbaum) or historians of the Revolution (such as Lefebvre or Albert Soboul), become more "Neo" and less "Marxist," it becomes difficult to distinguish their theoretical position from ordinary good sense. In the *Communist Manifesto*, we find a simple (or simple-minded) view of both the French Revolution and revolutions in general. Social structures (such as "feudalism") are built by ruling classes (such as the nobility) to protect their collective interests, only to be overthrown by rising groups (such as the bourgeoisie), both groups being defined simply by their productive roles. Neo-Marxism, however, has moved to the mere expectation that we will find in history social groups, variously defined, struggling for power and, thereby, continuously changing social structures.

Like Cobban, Eisenstein presents Lefebvre's views as if he were a simple-minded Marxist, who shows the "bourgeoisie" as having created the Revolution (or seized control from the aristocracy) in its own interests. Since I, on the other hand, have found his writings the most important impetus to the de-Stalinization of our view of the Revolution, at least one of us, clearly, is reading his works incorrectly. At a time when the non-Marxist Crane Brinton was excluding data on the prevalence of Jacobin Clubs in rural areas on the grounds that peasants only did what they were told by representatives on mission, Lefebvre was, for the first time, placing the peasant on the revolutionary stage as an actor in his own right, responding reasonably

¹ Georges Lefebvre, *The Coming of the French Revolution*, tr. R.R. Palmer (New York, 1947); review article by Elizabeth L. Eisenstein, "Who Intervened in 1788? A Commentary on *The Coming of the French Revolution*," *American Historical Review*, LXXI (Oct. 1965), 77-103.

² Alfred Cobban, *The Social Interpretation of the French Revolution* (Cambridge, Eng., 1964).

to the pressures of his own social, economic, and political situation. Again, while he gives perhaps more importance and less definition to the bourgeoisie than this group warrants, Lefebvre's primary emphasis is clearly upon the fact that, at its various stages and in various ways, all of the groups constituting eighteenth-century France somehow participated in the making of the Revolution.

A learned friend has recently described Soboul's Marxism as a kind of frame. The picture that he provides of the Revolution can be regarded as valid, valuable, even beautiful, but it is placed in an inappropriate Marxist frame. The frame does not detract from the picture, which could be removed from this frame and placed in another without changing its value.

The analogy suggests that the broad theoretical assumptions and the significant conclusions of the work are radically divorced from its empirical details. Since, in the social studies even more than in the fine arts, the frame really matters, as it provides the linkage between any particular historical study and those general views of man and society that justify the scholarly effort, Cobban and Eisenstein perform for us a truly valuable service. They are strongest when (in the classic tradition of Anglo-Saxons dealing with continental thought) they confront a simplistic Marxist statement with historical fact. For example, while he was certainly aware of it, Lefebvre neglected or refused to draw the full historical lesson from the prominence of nobles and clergy in the leadership of the struggle for doubling the representation of the Third Estate, during the winter of 1788-1789. Or, again, instead of assuming (as do both Cobban and the Marxists) that those who profited most from the Revolution must be those who produced it, Eisenstein insists that we approach these as two separate empirical issues, with proper respect for the unanticipated consequences of purposive social action, or the "ironies of history."

Two concerns, however, restrain my enthusiasm for these achievements: the danger that, in stereotyping Lefebvre's, before a superior synthesis is presented, we will feel that we need no longer read him (any more than we still read Marx); and the even more important danger that we will accept an alternative theory of history, with its associated methodology, as misleading as the Marxist view, which is introduced in the implicit assumptions of the critique. The critics' views of Lefebvre may be misleadingly partial, and their underlying ideas of social change and their views of historical method may simply lead us into new errors, or, what would be as bad if not worse, sterile negations.³

³ Unlike Eisenstein, Cobban explicitly regards Lefebvre as a "Marxist." (*Social Interpretation*, 11.) The difference, however, is only a superficial one in the etiquette of intellectual debate in England and the United States. While he uses the term "Marxism" or (worse)

We have already referred to Lefebvre's introduction in his early work of the peasant as an independent actor on the revolutionary stage. His *Grande Peur* and *Paysans du Nord* had some importance in the conception of those recent works by Paul Bois⁴ and Charles Tilly,⁵ which have cured us of thinking of the peasant of the west as a rural superstitious dolt simply following the rule of nobles and parish priests. They force us now to analyze his behavior as a political man in light of such objective conditions of his social and economic life as the purchase of land by the urban rich. This is hardly a Marxist approach in any narrow sense. It attributes mind and historical significance to the peasant, which Marxism has never done in theory or practice. It sees the process of urbanization as significant as productive relations, and, in the determination of the lines defining historically relevant groups, it takes residence as being equally as important as ownership of the means of production.

Lefebvre's later work seems to me to have been aimed at an empirical synthesis, rather than a *parti pris*. He can probably be more effectively criticized for having presented a flat, eclectic picture, without indications of the relative importance of the various historical events and processes he describes and analyzes. Non-Marxist positions abound in this mélange: for example, "France remained a nation of agriculture and handicrafts. The development of capitalism and of economic freedom met strong resistance on French soil"⁶ could have been written by Cobban. Perhaps *The French Revolution* is the only text in which we could find an author using both Augustin Cochin's study of the conspiracy of privileged groups in Brittany⁷ and C. E. Labrousse's analysis of the *conjoncture économique* at the dawn of the Revolution.⁸ Elsewhere, he expresses great admiration for Alexis de Tocqueville⁹ and even Hippolyte Taine.¹⁰ It is precisely his breadth, which encompasses the results of serious empirical work of any school, that provides, within his own work, the ammunition for critics of his general position regarding the importance of the bourgeoisie.

"sociology" to attack Lefebvre's fundamental theoretical approach, she speaks of "a static framework derived from a structural analysis," a description that, to my mind, bears no resemblance to anything in Lefebvre or Marx. The point is, however, that they seem identical in their ideas of what Lefebvre thought of the Revolution: as a successful effort by the bourgeoisie to break the bonds of feudalism so that capitalism could flower.

⁴ Paul Bois, *Paysans de l'Ouest: Des structures économiques et sociales aux options politiques depuis l'époque révolutionnaire dans la Sarthe* (La Mans, 1960).

⁵ Charles Tilly, *The Vendée* (Cambridge, Mass., 1964).

⁶ Georges Lefebvre, *The French Revolution from Its Origins to 1793*, tr. Elizabeth Moss Evanson (London, 1962), 33.

⁷ *Ibid.*, 101.

⁸ *Ibid.*, 116.

⁹ Georges Lefebvre, "Introduction" to Alexis de Tocqueville, *L'Ancien Régime et la Révolution*, ed. J. P. Mayer (Paris, 1952).

¹⁰ Georges Lefebvre, *La Grande Peur de 1789* (Paris, n.d.), 1.

Without some organizing principle, such an eclecticism would leave the reader disoriented, with a mass of facts and interpretations, but no clear ideas of the meaning, the sources, or the consequences of the Revolution. Lefebvre finds the mortar to hold his structure together in two distinct themes. The first is the retention of the Marxist view of the overriding significance of the bourgeoisie in a drama having many other participants, the primary point of criticism of Eisenstein and Cobban, to which I will return presently. The second, which cannot be ignored in any estimate of Lefebvre's intellectual orientation, or his relationship to Marxism, is the set of ideals of the Revolution, which, though formulated by the bourgeoisie, aimed at the universalistic assurance of the Rights of Man and the Citizen. While analyzing the contents of the Declaration in terms of the interests of those who drew it up, and of their constituents, and the historical circumstances and pressures of the moment, Lefebvre ultimately turns to its abstract, universalistic message as the profound meaning of the Revolution, even assimilating it to the Christian tradition: "The Church promised salvation to all without distinction of race, language or nation. To this universalism the new thinkers remained faithful. They secularized the idea of the Christian community, but they kept it alive."¹¹ Lefebvre takes no pains whatever to resolve this idealistic view of the meaning of the Revolution with his idea of the prominent role played by the bourgeoisie, perhaps because he saw no contradiction in the notion of a particular group as the agent of a universalistic ethic. Written at the impending death of the Third Republic, the closing passage can hardly be taken as the work of a simplistic Marxist: "It is therefore more difficult to live as a free man than to live as a slave and that is why men so often renounce their freedom; for freedom is in its way an invitation to a life of courage and sometimes of heroism, as the freedom of the Christian is an invitation to a life of sainthood."¹²

Lefebvre was certainly a Marxist, but he was also a demographic determinist, a constitutional theorist, an intellectual historian, and a humanistic moralist. He was, above all, an empiricist, and the pity would be if an attack on one facet of his work, however valid, should be taken as a basis for rejecting or, worse, ignoring the rest.

The validity of the attack is, however, still on the agenda. Eisenstein and Cobban conclude that there is no basis for the Marxist description of the Revolution as an act of a "bourgeoisie" in opposition to the restrictions of a "feudal" system. Taken as a dogma (as it sometimes has been), this is unacceptable, but, taken as a hypothesis, I believe that the necessary theoretic-

¹¹ *Id.*, *Coming of the French Revolution*, 183.

¹² *Ibid.*, 187.

cal and empirical work for its evaluation remains to be performed. This work would be built, as are the critiques of Cobban and Eisenstein, around a limited number of questions: What is the "bourgeoisie"? What is "feudalism"? What is a "revolution," and what is "social change"? And what is to be accepted as historical evidence?

The critics raise questions about both the identity and the historical role of the bourgeoisie. Both seem particularly incensed at the use of a concept referring to people of highly varied occupations, wealth, and social origins. Lefebvre himself makes clear the heterogeneity of the French bourgeoisie, in contrast, for example, to that of Russia, where Catherine was apparently able to identify it clearly enough to grant it corporative autonomy and exemption from military service. The diffuse character of the concept, as used both by contemporaries of the Revolution (such as, notably, Antoine Barnave) and by Marxists and those they have influenced, leads Cobban and Eisenstein almost (but not quite) to wish to do without it entirely. At one point, Eisenstein is carried away:

Finally, to apply the term "bourgeois" to village priests as well as to "the upper level of the nobility . . . whose conditions of life drew them to the bourgeoisie" . . . is to stretch this much-abused term beyond its already frayed limits. There is something wildly askew about a structural model that includes the top layer of nobles and the bottom layer of the clergy within the middle ranks of the middle class.¹³

If the son of a bourgeois family enters the clergy, the clergy is not thereby "bourgeois," nor does Lefebvre ever designate it as such. As for the nobility, the full quotation reads: "At its upper level, the nobility *tended to suffer amputation of a minority* whose conditions of life drew them to the bourgeoisie and gave them liberal ideas."¹⁴ I have italicized the passage that fills the ellipsis in her quotation because it is important. To say that a minority (Mirabeau may serve as an example) is amputated is not to say that the nobility, or any part of it, is to be included in the reference of the concept of the "bourgeoisie." It is to say the opposite: that having entered the bourgeoisie, the minority is no longer considered in law or public opinion as "noble." In both the case of the priest (who, after all, had to come from somewhere outside the clergy!) and the *noble dérogré*, Eisenstein confuses the origins of an individual with the structural position of a group. Her differences with Lefebvre here are not over historical facts, but over the proper procedures for the construction of social concepts and the analysis of social events.

Since the "bourgeoisie" contains a wide variety of groups (although not

¹³ Eisenstein, "Who Intervened in 1788?" 94-95, n. 27.

¹⁴ Lefebvre, *Coming of the French Revolution*, 14.

as wide a variety as Eisenstein thinks), it is certainly, from one point of view, difficult to define. But that which is difficult to define may, in fact, exist, and even have great historical significance. Indeed, the lack of precise definition may even be an important part of the historical situation: Tocqueville makes much of the lack of *cultural* differentiation in France between the upper levels of the Third Estate and the nobility. "They differed only in their rights" is his summary of a situation he regards as explosive. This he contrasts with developments in England where the social, legal, and kinship lines between noble and commoner were more ambiguous, and with Europe east of France, where differences in culture and style of life rendered stratified groups more identifiable, and differences in prerogatives presumably more acceptable. I suggest that, in this context, both contemporaries and Marxists have generally meant by the "bourgeoisie" nothing more complex, nor better defined, than the "upper levels of the Third Estate," which is to say the wealthier, more urban, more educated, less privileged members of the society. I believe that such a definition is workable since we can usually identify those who belong to the group and proceed to study the role of such people in the events of the Revolution.

One can readily see their very minor role in the *leadership* of the movement that intervened in the conflict between the aristocracy and the monarchy in the winter of 1788 to insist upon the doubling of the representation of the Third Estate. But this does not mean that they did not play an important role. Eisenstein is correct in emphasizing that those who made the Revolution need not have been those who profited from the Revolution, but I would go one step further: those who, at any given moment, led the Revolution were not necessarily those who made the Revolution. This point bears upon the more general question of the contexts of political action as relevant historical material, to which we will return below.

While Eisenstein's critique is directed almost exclusively at the identity and significance of the "bourgeoisie," Cobban also takes up the nature of the "feudalism" against which, in the Marxist view, the bourgeoisie are supposed to have taken up arms. His position is unambiguous: "If 'feudalism' in 1789 did not mean seigniorial rights, it meant nothing."¹⁵ And since, Cobban continues, these rights were destroyed by action of the peasants against the wishes of the bourgeoisie, many of whom had vested interests in them, there was no bourgeois revolution against feudalism. *Quod erat demonstrandum.*

One almost hesitates to examine so elegant an argument for fear of disturbing the peace. Like so many of the concepts used in the social studies,

¹⁵ Cobban, *Social Interpretation*, 35.

"feudalism" has had a varied history. Carl Stephenson, for example, refuses to use it unless he can visualize a man in full armor on a horse. But the term has been used much more loosely, by participants in the revolutionary events, by scholars, and by political activists with a large number of pejorative connotations and institutional denotations.

As a conscientious scholar, Cobban inquires: in the institutions of eighteenth-century France, what remains with sufficient historical continuity with the medieval system of land tenure and its associated obligations, to warrant the application of the term "feudalism"? He finds only seigneurial rights and privileges. But this, whether correct or not, is irrelevant to his purpose, which is to evaluate the "social" or Marxist interpretation of the Revolution. For that purpose what we need is not a historically justifiable definition of feudalism for the eighteenth century, but rather an idea of the meaning intended by those, like Marx, Jean Jaurès, Albert Mathiez, Albert Soboul, or Daniel Guérin, at whom the critique is aimed.

For these people, I am sure, the term refers to all those institutions of the old regime providing special rights, privileges, or powers to the first two orders of the realm, or to privileged groups of commoners, including (besides seigneurial rights) privileges in legal processes (such as *committimus*); exclusive access to careers in the Church, the military, and the diplomatic corps; deferential rights to church pews, the wearing of swords, and the use of weather vanes; recreational privileges such as the rights to hunt, fish, and keep pigeons and rabbits; the rights of assembly and political representation; and, perhaps most important, tax privileges, exemptions, and advantages. The fact that many of these, the seigneurial rights particularly, came in later years into the hands of *roturiers* does not change their designation as "feudal."

What is important in the Marxist view is not the historical sources of these social arrangements in medieval life, but that, along with such later innovations as venal offices, royal grants and pensions, tax farming, and governmental and guild restrictions on freedom of production and distribution, they functioned inappropriately for the demands of the "capitalist" (read "modern") world. Where they are most wrong, and Cobban's critique is most powerful, is not here, but in identifying the needs of the modern world, to which the "feudal" old regime institutions were inappropriate, with the demands of modern industrial production. The evidence indicates that these needs are to be found much more readily in the consequences of agricultural innovation, demographic pressures, urbanization, and foreign and colonial trade, with their associated military adventures and fiscal pressures.

Cobban denies the charge he attributes to Lefebvre, that he had intended to deny the existence of the Revolution in his inaugural lecture.¹⁶ If a man is the final authority on his own intentions, and if Lefebvre did make such a charge, we must accept that Lefebvre made a mistake; but it was a mistake easily made. At every step in his argument Cobban takes pains to show the similarities of postrevolutionary France with the old regime. For example: "Looking at the economic consequences of the revolution as a whole, they seem astonishingly small for such a great social and political upheaval."¹⁷ This conclusion is reached by the examination of data on the rate of industrialization and trade during a period of twenty-five years of internal and external warfare. I find it hard to understand how he could write: "Finance, in fact, traversed the revolution little changed except in personnel,"¹⁸ unless, somehow, the unleashing of the assignats on a society profoundly distrustful of paper money since the John Law debacle could be regarded as unimportant. He doubts that there was a "permanent change of personnel in the upper ranks of society."¹⁹ To investigate this, "It would be interesting to know to what extent, in different parts of the country, the noblesse kept its lands during the revolution. . . . We know also that there were many purchases of *biens nationaux* by nobles, sometimes even on behalf of *émigrés*. . . ."²⁰ Also, the abolition of venality of office is not to be taken too seriously because the officers were compensated, and, "Moreover, many of the former *officiers* seem subsequently to have obtained salaried judicial and administrative positions not dissimilar from those for the loss of which they had earlier been compensated."²¹

I would hold that a society that has changed from one in which industry and commerce are regulated by a multiplicity of Colbertian mercantilist regimens and administrative agencies as well as privileged corporate guilds and in which internal trade is hampered by a multiplicity of prohibitions and tariffs, to a free national market economy, has undergone a fundamental change—a revolution—even though, temporarily, industry and trade do not expand under the pressures of continual warfare. Even if every noble becomes a landlord in fee simple, I would contend that a revolution has occurred in the stratification system of the society. The same biological humans, or their descendants, are to be found in leading positions, but they are different social beings, with different rights, duties, and functions: they pay taxes like everyone else (or avoid them under the same rules as

¹⁶ *Ibid.*, 81.

¹⁷ *Ibid.*, 77.

¹⁸ *Ibid.*

¹⁹ *Ibid.*, 83.

²⁰ *Ibid.*, 87.

²¹ *Ibid.*, 84.

everyone else); they appear in the same courts (even though with better attorneys); they sit in legislative assemblies if elected, not in constitutive assemblies by right of birth; if they wear different clothes or enjoy better career chances, it is because they command more resources and not because they are given monopolies in law or by discriminatory administrative practice. Finally, a society with a bureaucratic administrative staff, responsible in a hierarchy to central authorities, is a very different society from one in which many of those who must be charged with the day-to-day administration of executive decisions hold property in office, even if all the biological individuals filling the bureaucratic slots were once venal officers. (Ask Turgot, or Napoleon; or read Max Weber.) Cobban never claims that there was no French Revolution, but Lefebvre's charge is essentially sound; he gives us a large number of specious reasons to believe that *plus ça change, plus c'est la même chose*, based largely upon a wholly inadequate theoretical conception of what constitutes social change.

My final point, directed more at Eisenstein, is perhaps the most important, since it bears upon the most general questions of historical method. In the closing paragraphs of her critique she takes a methodological position which, I believe, is directly contrary to the very idea of a social history. She attacks both conspiracy theorists and "those who insist on spontaneous mass or class action" as equally prone to "ignore the real men who formed and led the patriot party. . . . In both instances we are asked to look around, over, beyond, above, or below rather than at the assorted individuals whose group action we are curious about."²² This passage only makes explicit the underlying methodological source of a number of serious errors earlier in her paper: her unwillingness to examine the social contexts of political action while pursuing her favored (and necessary) method of "collective biography."

She writes, for example, "Up to this point [September 23, 1788] the 'aristocratic revolution' was proceeding without intervention from other social sectors. . . ." But during the June crisis following the Lamoignon edicts suppressing the parliamentary powers, there were "popular" disorders in Toulouse, Dijon, Pau, and, notably, Grenoble (the "Day of Tiles"). Nobles rarely riot. From the point of view of social history, this is "intervention" of a most important sort. Since she cannot see this as "intervention," she can cite the Vizille assembly, at which the Third Estate was given the vote by head with double representation and fiscal equality, as an instance of liberal provincial aristocratic "initiative."²³ She is puzzled²⁴ by this action,

²² Eisenstein, "Who Intervened in 1788?" 102.

²³ *Ibid.*, 79, 84.

²⁴ *Ibid.*, 84, n. 11.

since the same people had earlier rejected similar forms proposed by Brienne for the new provincial assemblies. Perhaps her puzzlement would disappear if she would look not only at the ideas and group memberships of the people who signed the papers, so to speak, but also where *they* looked, around them, at the actions of those with whom they were allied, or whom they feared. In fact, the "Day of Tiles" took place at Grenoble only six weeks before the Vizille assembly, a fact that we may not ignore any more than could the aristocracy of the Dauphiné.

The fact that all political actors (and not only elected delegates) have, in a sense, constituents goes far to explain those occasions in which we find them taking actions that violate their deepest personal convictions. Sometimes, as in the case of the Vizille assembly, since the constituents are off stage, we must proceed by comparing the plausibility of their influence with the fantastic notion that the assembly was unaware, or uninterested, in the violence in the streets only six weeks earlier. As Kaplow shows, however, in another case she is puzzled because her methods blind her to the relevance of a literal constituency physically present in the assembly hall: the democratic position taken by the privileged representatives of the Third in the provincial estates of Brittany.

This inability to see political actors in relation to their constituencies, and not merely as expressing their personal beliefs, leads to some strange interpretations of the convocation. The elections are said to have been "partly rigged" because only a handful of *bailliages* chose to send nobles or clergymen as representatives of the Third.²⁵ The privileged orders were "eliminated from the running."²⁶ But this was a choice of the assemblies. They were eliminated from the running because they were slow horses, not because they were unfairly scratched. The fact that the *cahiers* contain demands for a legal restriction that the delegates of the Third be chosen from members of the Third is irrelevant; such demands must necessarily refer only to future convocations. Finally, Eisenstein seems to be led to a most astonishing confusion between the statistical concept of "representation" and the political concept. "The nobles and priests who retained their seats may have represented an infinitesimal minority of the population. The literate commoners did not represent a much larger one."²⁷ This is nothing more than a bad pun. The delegates of the Third represented over 95 per cent of the population, which they did not resemble any more than Ted Kennedy resembles me. They were given *cahiers* expressing the desires of their constituencies and, at times, mandates limiting their range of choices in legisla-

²⁵ *Ibid.*, 96.

²⁶ *Ibid.*, 95.

²⁷ *Ibid.*, 97.

tive actions, and committees of correspondence to keep them responsive to the desires of their constituents. It was undoubtedly the will of the vast majority that the delegates be atypical precisely in such qualities as literacy and knowledge of public affairs in the hope that they would serve the interests of their constituents not only more reliably than a noble but also more competently than a *maître Jacques*.

A proper appreciation of the relevance of social contexts in the formation of political decisions would reveal to Eisenstein the possibility of a high degree of convergence in the types of delegates chosen, and of grievances expressed, without any central direction or undercover campaign. Hence, I cannot agree with the theory underlying her claim that, "However enterprising they may be, men who are located 'everywhere' simply cannot take 'concerted action' to steer an electorate toward a given slate of candidates. One group located in one place is required to see that all the others do not 'steer' in all directions."²⁸ Indeed, unless local communities and their leaders faced, to some degree, common situations and problems, there is little reason to expect them to pay attention to Parisian opinions in their choice of delegates or model *cahiers*. In fact, many did not. Eisenstein quotes, but nevertheless ignores, Lefebvre's statement that most models were drafted locally.

The most serious of those errors deriving from her unwillingness to examine the contexts, and, particularly, the constituencies, of political actors is her erroneous interpretation of the events of the night of August 4. She explicitly regards the abolition of "all privileges" on this date as the work of a limited number of leaders of the patriot party who cut across the lines of estates and social classes.²⁹ In this case, the facts are established and clear. The dramatic gestures of the privileged on August 4 only endorsed a *fait accompli*; to a great extent their privileges had already been destroyed by peasant uprisings, municipal revolutions, and the Great Fear—disorders in which, among other things, the records of seigneurial obligations were often destroyed. The decision to abjure that which they had already lost was made the previous night in the Breton Club, where over a hundred deputies participated, most of whom were recruited from the upper levels of the Third Estate.³⁰ Naturally, if this particular gesture, designed to re-establish civil order and political stability, were to have effect, it must be set off by the action of those with the status of the Duc d'Aiguillon or the Vicomte de Noailles. Eisenstein takes these symbolic figures as independ-

²⁸ *Ibid.*, 94, n. 26.

²⁹ *Ibid.*, 100.

³⁰ Lefebvre, *Coming of the French Revolution*, 129–30.

ent actors, ignoring both the caucus that obviously prompted them and the mobs that prompted the caucus.

A similar analysis would explain civil rights legislation today by the actions of a small, socially heterogeneous group of students, ministers, congressmen, and Supreme Court justices, with nothing in common except the reading of the Fourteenth Amendment, affected neither by urban crime nor by protests in the streets of Watts, Harlem, Selma, Philadelphia, Rochester, or Chicago. Malcolm X actually feared such an interpretation. He once asked his autobiographer, Alex Haley, to deliver a message to the latter's brother, a state senator. "Tell your brother for me to remember us in the alley. Tell him that he and all of the other moderate Negroes who are getting somewhere need to always remember that it was us extremists who made it possible."³¹ The continuing task of the social interpretation of the French Revolution, to which Lefebvre's contributions remain of first importance, is to find the links between the actions of those in the alley and those in the palace.

³¹ *Autobiography of Malcolm X, with the Assistance of Alex Haley*, as quoted in *Times Literary Supplement*, June 9, 1966, 507.

III

A Reply

ELIZABETH L. EISENSTEIN*

MR. Kaplow and Mr. Shapiro have raised several issues that need further clarification. I wish they had looked harder at the particular issue raised by my "Commentary." Where they do touch upon it, they stay in the provinces; although, in following Lefebvre, I focused on Paris. In the provinces, moreover, they skip over the places where, following Lefebvre, I spent some time (Dijon, for example). That I was following Lefebvre's account and not making up my own, scrutinizing a single text and not synthesizing many, has been overlooked. Let me restate my purpose. I did not set out "to show how a traditional Marxist approach . . . is contradicted by the growing body of evidence" or to compare this evidence with Lefebvre's approach (however labeled). As both my title and first paragraph make clear, I did try to show how presentation of a strategic point in a single influential

* Mrs. Eisenstein is the author of "Who Intervened in 1788? A Commentary on *The Coming of the French Revolution*," *American Historical Review*, LXXI (Oct. 1965).

book was contradicted by the author's own evidence. In view of this stated purpose, I am unrepentant about the second of my two sins detected by Kaplow and wish to lodge a complaint instead. By taking my exegesis as if it pertained to something other than a particular text, both critics have smudged the clearly defined limits of my commentary and blurred the sharp focus I sought to obtain.

My remarks about a "static framework" are thus applied (by Kaplow) to a "theory of class." He goes on to object that this theory theoretically makes room for evolution and complexity. Shapiro makes a similar objection. My remarks did not pertain to any abstract concept or to the use of it made by other historians, or by Lefebvre, himself, in his other works. They referred to how a particular narrative describing political action was interrupted at a crucial point by a chapter devoted to class structure, thereby breaking the thread of the narrative, distracting attention from a group that supplied continuous leadership, and artificially separating members of this group. I see nothing contrived or theoretical about this point and wish Kaplow had discussed it. In the guise of objecting to a "straw man technique" he has, instead, injected into my discussion several higher order abstractions—the very ingredients I tried to leave out. Whether the dichotomy noble-bourgeois is congruent with the dichotomy feudal-capitalist is certainly disputable. I would side with George Taylor and Betty Behrens on this dispute,¹ but that is beside the point. I happen to be more curious about the location of revolutionary initiative in 1788–1789. Here I did object (and still do) to making a "blank-faced bourgeoisie" responsible for initiating action. But that class membership automatically determines political responses or that noblemen act like Pavlov's dogs was not imputed by me to Lefebvre. Such straw men are not of my making any more than are the passages I cite from Lefebvre's book.

Fortunately Kaplow does get down to specifics. His tactics remain evasive, however. In dealing with the protest movement he passes over Parisian leadership, beginning instead with unevenly documented provincial developments. Surprisingly, he seems to agree with my speculations about socially heterogeneous local leadership. He then states, as if posing an objection, that some bourgeois did intervene in numerous provincial towns. But this seems entirely compatible with heterogeneous local leadership. While they were not alone, he goes on, these bourgeois did constitute a

¹I have not read Mr. Taylor's article in this issue and refer to the paper delivered at San Francisco: George Taylor, "The Bourgeoisie: Proposals for Clarification," American Historical Association Annual Meeting, Dec. 30, 1965. Betty Behrens, "Nobles, Privileges and Taxes in France at the End of the Ancien Régime," *Economic History Review*, XV (No. 3, 1963), 451–75, came to my attention after my "Commentary" was published, as did her review article mentioned below. Both independently confirm my views.

new element in the struggle. Now this is an argument that is not in Lefebvre's book and hence was not discussed in my commentary. It should nonetheless be considered. I would hold that some bourgeois had also previously been active in the so-called "aristocratic revolution."² The new element in the struggle was, I still think, the injection of a new issue: defiance of the Paris Parlement's ruling. The social composition of groups responsible for mobilizing opinion on this issue has, in my view, some bearing on theories about the "class nature of the struggle of 1788." Why should evidence on this point prove "nothing whatsoever" about such theories? If a theory is so framed that it cannot be invalidated by any evidence, I regard it as useless and barren. At all events, I welcome Kaplow's use of my own argument that many traditionally submissive commoners had to be roused into action even if he finds me "curiously blind" in the process.

Space limitations require a very cursory review of other points raised by Kaplow (I am following his order throughout). 1. The first estate is simply not equivalent to the second in any century. It compounds confusion to regard it as such. 2. The "guts" were not removed by a tactic that made passing the resolution possible, since the gut issue was doubling at the time the section voted. That it was a shrewd tactic not to press voting by head is persuasively argued by Lefebvre.³ 3. Objections to the view that patriot leaders favored the separation of orders to prevent commoners from electing privileged persons should be directed against Lefebvre's account. Presumably, he had "shreds of evidence" in mind when he wrote the passage I cited. 4. I point to contradictions on many more than two occasions. The particular footnote singled out by Kaplow contains another more important puzzle than the one he has unwittingly misconstrued.⁴ 5. The second contra-

² E.g., the "lawyers" and "shopkeepers" mentioned by Lefebvre, *The Coming of the French Revolution*, tr. R. R. Palmer (Princeton, N. J., 1947), 33-34; the groups around the Palais de Justice noted by Jean Egret, *La pré-révolution française (1787-1788)* (Paris, 1962), 154-55, and similar groups in Grenoble he notes elsewhere. (See note 13, below.) "Active support and participation" of the Third in the "révolte nobiliaire" is documented by Ralph Greenlaw, "Pamphlet Literature in France during the Period of the Aristocratic Revolt (1787-1788)," *Journal of Modern History*, XXXI (Dec. 1957), 353.

³ Lefebvre, *Coming of the French Revolution*, 55. The more detailed account of passage of the resolution given by Egret, *Pré-révolution*, 343-44, agrees with my interpretation, not with Kaplow's.

⁴ See my "Who Intervened in 1788?" 83, n. 10. The major puzzle was how a center of parliamentary resistance became a center of resistance to parliamentary authority. The minor one involved the Breton Third Estate portrayed earlier as composed exclusively of nobles and privileged persons but later as having "*long* demanded" fiscal equality. Both critics miss this contradiction (possibly I should have italicized "*long*" in my original citation). Both object that I missed seeing how pressure groups forced the "privileged" deputies to defy the first and second estates in late December 1788. This defiance, however, involved new political as well as old fiscal demands. These old demands for fiscal equality, I repeat, come oddly from an assembly of "nobles" until one realizes that municipal oligarchs are not really equivalent to nobles. Here,

diction he mentions does involve a major issue, and I hope more thought will be given it. The spreading of news about the Parlement's ruling seems to me entirely relevant to the "organizing of revolution." Given the way news was circulated in eighteenth-century France, references to "newsboys" seem anachronistic.⁵ Kaplow's distinction between "clandestine" and "legal" news distribution does not hold up.⁶ The private couriers and agents of the Parisian leaders had access to the same transportation whatever messages they carried. According to Egret, Volney's *Sentinelle du Peuple* "gave the real signal for the bourgeois revolution" in Brittany. This was not a clandestine periodical. Indeed Bretons thought Volney was a government agent.⁷ But Volney had been dispatched to Rennes by the Parisian leaders, according to Lefebvre.⁸ 6. An objection to the inclusion of parish priests, often recruited from the peasantry, within the category of "enterprising bourgeois" is not, I think, a quibble. 7. Of course one cannot discuss the making of the Revolution solely in terms of who was responsible for a given set of actions, whether one considers the "whole Revolution" or any of its less problematic phases. I pointed this out myself. The question is: should one discuss the making of the Revolution (in whole or in part) by excluding the problem of who was responsible for initiating action or by disregarding evidence pertaining to this problem? 8. If I have failed to make my point, I wish Kaplow would show me how or where. The discursive footnote (about a skewed model) he goes on to discuss is clearly not the place to look.⁹ 9. His peroration significantly rephrases a statement made previously

as elsewhere, Egret's treatment resembles Lefebvre's. Compare his description of the mayor of Nantes as a noble who led the privileged Nantais faction in "The Origins of the Revolution in Brittany (1788-1789)," in *New Perspectives on the French Revolution: Readings in Historical Sociology*, ed. Jeffry Kaplow (New York, 1965), 142, with description of the same man as one of five *roturiers* attending an Assembly of Notables in *Pré-révolution*, 341.

⁵ See account of news distribution and "chambres de lecture" in Augustin Cochin, *Les sociétés de pensée et la Révolution en Bretagne* (2 vols., Paris, 1925), I, 20. Egret, while arguing with Cochin (much as Aulard did with Taine) draws often on this well-documented study.

⁶ The number of days that elapsed before the ruling from Paris became official was eighteen in Brittany, twenty-nine in Provence, thirty-one in Toulouse. (Egret, *Pré-révolution*, 347, n. 1; also in *Readings*, ed. Kaplow, 140, 159.) How long did it take to travel by unofficial channels, and how was it conveyed as it did so?

⁷ Egret, "Origins of the Revolution," 151, 141.

⁸ Lefebvre, *Coming of the French Revolution*, 52.

⁹ Both critics have misfired in taking this footnote as a target. (See 94-95, n. 27, of my "Who Intervened in 1788?") Shapiro hauled out the heaviest artillery and made the worst blunder. I deliberately left out the ellipsis he fills in to avoid misleading a reader who might not consult Lefebvre's whole paragraph. Presumably, Shapiro did consult the paragraph. He was nonetheless misled by the phrase "amputation of a minority." It does not apply to the "lower level" where "the nobility also suffered from attrition," unclassing themselves "like Mirabeau" ("déclassé" is, incidentally, confused by Shapiro with "dérogé"). The "amputation" excludes Mirabeau *et al.* and refers to the "upper level," that is, to rich prestigious lords who acquired "liberal ideas" and English Whiggish ambitions. (See Lefebvre, *Coming of the French Revolution*, 14.) Surely a "skewed model" results when a group exhibiting no signs of downward mobility and personifying the "haute noblesse" for contemporaries is arbitrarily amputated by a later historian. Kaplow is merely wrong in saying I lumped the top layer of nobles with

in his introduction to his volume of readings. There (version *A*) he holds the Revolution was bourgeois "because the bourgeois initiated it and emerged triumphant from it."¹⁰ Here (version *B*) he says, more ambiguously, it was because the bourgeoisie was "active . . . had ideals . . . [and] were dominant." To make my position clear, I will comment on these two versions. *A* contains two statements that can be detached and separately tested. Reserving judgment on the question of outcome, I find that the evidence does not support the assertion about initiative. *B* offers an opening for further discussion, namely, when, how, or by what means did commoners become politically dominant. Finally, I share with Kaplow the desire to "go on from there," but am puzzled by his phrase "at long last." As Behrens points out, the sites to be dug for the foundation he mentions were already prospected by Barnave and Sieyès.¹¹ Historians have been going "on from there" ever since and will undoubtedly continue to do so for many more decades. It is only those of us who are dissatisfied with the results of this work—seeing not a "wave of progress" but a proliferation of false issues—who are deterred from contributing further to it.

This brings me to Mr. Shapiro's excursion into the field of French revolutionary historiography. I have no space to unravel the tangled web he has woven, but must express dismay at having my article set within such an inappropriate context. Fortunately Behrens has provided a different, more suitable one. In so far as my piece enters into his account, Shapiro begins by misconstruing my main purpose. He goes on to opine that I read Lefebvre's works incorrectly without knowing my views on these works. Had I chosen to appraise the lifework of a prolific historian instead of analyzing portions of an influential text, I would have written a very different article. Even while he neglects the French historiographical tradition upon which Lefebvre draws and the Robespierist cult that runs, like a red thread, through his work, Shapiro worries about my exaggerating the "Marxist" element in Lefebvre's *œuvre*. In fact, he stresses it much more than I would—and clearly much more than I did in the actual article I wrote.¹²

Shapiro asserts that this article contains a "number of serious errors" all

the bourgeoisie on the basis of political ideas, since Lefebvre did this. In lecturing me on why I am confused about a point he has muddled, Shapiro is infuriating as well as wrong. As for parish priests, the "quibble" noted in point 6, above, accounts for my assertion (queried by Kaplow) that Lefebvre placed them among bourgeois merely because they were literate. Shapiro somehow misconstrues my remarks about recruitment and ignores my basis for asserting that Lefebvre places them among "enterprising bourgeois."

¹⁰ Kaplow, "Introduction," *Readings*, ed. *id.*, 13.

¹¹ Betty Behrens, "'Straight History' and 'History in Depth': The Experience of Writers on Eighteenth-Century France," *Historical Journal*, VIII (No. 1, 1965), 117, 120.

¹² The name "Marx" appears in a single citation; the "ism" does not appear at all.

derived from a presumed blindness to "social context." Since I had tried to avoid errors and had looked at social context, I was troubled by this indictment until I examined the counts upon which it was based. To put the matter bluntly, he has blundered too often in posing his objections to make possible a fruitful debate. One blunder, based on careless reading, has already been described (see footnote 9). Another involves his bringing up the "Day of Tiles" without first doing homework in straightforward political history. His presentation of this episode points to the fallacy of ascertaining "methodological positions" before posing simple questions such as: who? what? when? where? how? The Grenoble *émeute* of June 7, 1788, as described by Egret, was instigated by judicial aids to protest royal action against the local parlement—a popular protest in a town economically dependent on legal business. It formed part of the "aristocratic revolution," involved the issue of Versailles versus the provinces, and happens to support my footnote on the Vizille assembly. According to Egret, it had no repercussions outside Grenoble and "obtained no result"—a "fantastic notion" according to Shapiro. Egret views the Dauphinoise revolution as the work of a "coterie" led by Mounier. Shapiro asks me to look where members of the Vizille assembly looked. "The multitude never had any influence on our assemblies," remarked Mounier.¹³ The often cited remark: "I am their leader, I must follow them" may be applicable to 1848 but not to 1788. This possibility should be considered when distinguishing between those who led and those who made the Great Revolution. Historians cannot afford to be tone deaf to the music of time.

To "see political actors in relation to their constituencies" requires, first of all, finding out who the former were. Such an attempt, I am told "is directly contrary to the very idea of a social history." Any kind of history, in my view, is incompatible with premature leaps in the dark. A look at the composition of pressure groups formed within and without duly constituted bodies has convinced me that some premature leaps have been made in the wrong direction. How does it undermine social history to point this out? If I had dealt with the Breton assembly of December 1788 (and I did not) I would not confuse deputies with constituents, but would examine the different pressure groups that were present.¹⁴ Of course an argument

¹³ Jean Egret, *La Révolution des Notables: Mounier et les Monarchiens 1789* (Paris, 1950), 8, 9, 12, 17.

¹⁴ In Provence, according to Egret, revolutionary initiative was displayed by "members of the nobility not possessing fiefs." Instead of trying to decide whether they were "political actors," representatives, or "constituents," it seems preferable to look at them as a pressure group. It would be useful to know, in the Breton case, how the social composition of the recalcitrant faction in Nantes (led by a rich new noble) differed from that of the "revolutionary" faction (led by a rich new noble). I would single out three different pressure groups gathered in the hall who forced action from recalcitrant spokesmen for forty-two Breton towns: 1)

based on misconstrued footnotes is bound to engender unnecessary confusion, but difficulty with fine print is not the only source of such confusion. Shapiro has also misread my meaning to produce a "bad pun," divined the will of the peasantry in accordance with his thesis rather than historical veracity, and imputed to me his own "strange interpretations of the convocation."¹⁵

Unless degree is specified, it is meaningless to say that "local communities . . . faced, to some degree, common situations. . . ." Common denominators are required for modern social analysis. Historical imagination has to be exerted to prevent the search for such denominators from distorting conditions prevailing in earlier eras. Not neglect of social context but caution about historical context leads me to worry about such distortions when thinking, back across two centuries, about situations in *ancien régime* France. The possibility that "a high degree of convergence" could occur "without central direction" is one that I did not ignore. I examined it with some care when discussing the single demand that "sounded loudly" during the protest movement of 1788-1789.¹⁶ Shapiro ignores this discussion and accuses me of ignoring the subject I discussed. Having added insult to injury, he throws in a discourse on methodology for good measure.¹⁷

To find my "most serious" error he again passes over the events I did discuss and hits upon one I did not. He takes a glancing reference out of context from a long sentence about the group that "provided the basis for whatever unity or continuity may be perceived in the early phases of the French Revolution." The reference was, admittedly, too fleeting and should be emended: "who *helped to abolish* all privilege" conveys my meaning properly. This emendation does not alter my point that continuity of leadership was demonstrated by Lefebvre's account of the night of August 4.

twenty-nine deputy commissioners from different towns; 2) fourteen extraordinary deputies from Nantes; 3) a group of local "jeunes gens" who threatened to pelt uncooperative deputies from the galleries. (See Egret, "Origins of the Revolution," 161, 144.)

¹⁵ Elections held according to medieval legal fictions necessarily differ from those held in twentieth-century Boston. The Third deputies legally represented one order of the realm in 1789 as in 1614. In terms of modern concepts (whether statistical or political) these deputies did not represent 95 per cent of the population. They were not provided with legislative mandates by a preliterate rural populace and did not keep in touch with this populace by "committees of correspondence." My remarks about partial rigging were not based on election results or on demands concerning future convocations. They were based on a passage cited from Lefebvre which is, incidentally, supported by Egret, *Pré-révolution*, 360. *Cahier* demands are, indeed, "irrelevant" to my discussion.

¹⁶ See my "Who Intervened in 1788?" 91-92. That the Parisian leadership deliberately aimed at achieving "simultaneity" and uniformity is suggested by Egret, *Pré-révolution*, 331, 355. I cannot fathom why both critics discount evidence supplied by Lefebvre and Egret pointing to some measure of central organization.

¹⁷ I did not ignore Lefebvre's reference to locally drafted models, but went on to look at the groups he held responsible for drafting them. As for the footnote on steering an electorate, I will stand by it.

Target read the proclamation; Noailles was "in on the secret"; Aiguillon was already active as a member of the Committee of Thirty in the fall of 1788 and deserves, no more than any other of its members, to be regarded as a mere "symbol." The Breton Club grew out of a protest movement that had been initiated and orchestrated by Parisian leadership and was linked with this leadership.¹⁸ To detach members of its caucus from those who implemented its decision in public the next night or to regard the latter as mere figureheads manipulated by the former is to draw an unwarranted inference that does not agree with prior developments and has nothing to do with "facts that are established and clear." Neither the Breton Club caucus nor peasant uprisings provided the revolutionary movement of 1788-1789 with unity and continuity. A leadership that cut across estates and social classes probably did. Consistent to the end in ignoring this issue, Shapiro concludes by offering his views on the present civil rights movement.

In his discourse on methodology and social context, he has inadvertently misconstrued political history and misread Lefebvre's account. He has also falsified my argument, playing carelessly with textual context to do so. More important and dismaying, his whole paper—beginning with remarks about Albania and China, going on to de-Stalinization and Ted Kennedy, ending with Malcolm X's "autobiographer"—reveals a reckless disregard for historical context. I object to his stating that my position on any issue is "directly contrary to the very idea of a social history" as I object to his delivering his opinion of the Day of Tiles "from the viewpoint of social history." For one thing social history has no viewpoint, although social historians have many. For another, Shapiro is not a historian; he is a sociologist. There are many occasions where too much has been made of this distinction and some where it cannot be drawn at all. But this seems to me one of those exceptional cases where it should be drawn as emphatically as possible.

The most fitting summary of my general position on the objections raised by both critics has been provided by Behrens in her review of three recent French books. I will conclude with her remarks:

Whatever merits these procedures may possess there seems to be one overriding objection to them. It is the objection against all arguments based on imprecise concepts or on concepts which change their meaning in the course of the discussion. They yield no clear and verifiable conclusions. So much confusion has recently been introduced into French eighteenth-century history that the central doctrine of the class struggle between bourgeois and aristocrats can now only be ac-

¹⁸ On links between Breton revolutionary faction and Parisian leaders, see Egret, "Origins of the Revolution," 151.

cepted as an act of faith; for no two people can agree on who the bourgeois and the aristocrats were . . . and every argument is thus liable to be at variance with easily ascertainable facts.

In these circumstances it is plain that the conventional descriptions of the Ancien Regime and the explanations for its collapse need reformulating. . . . It would be utopian . . . to suppose that a reformulation will emerge of itself if only more people write monographs and more operators with paste and scissors attempt to tinker up the outlines in the light of their conclusions. What is needed are some clear and useful concepts by means of which to organize such information as exists and to show where fresh research can profitably be undertaken.

. . . whatever is meant by a scientific explanation, at least it is an explanation directly related to the situation to be explained, which in history is a political situation. It cannot be arrived at by adding together the conclusions of researchers who started from other terms of reference. Such an attempt, indeed, is incompatible with the use of a scientific method; for however that term is understood, at least it involves getting one's central concepts clear, and one's arguments coherent, and one's essential facts right, or at least not demonstrably wrong.¹⁹

¹⁹ Behrens, "Straight History," 125-26.

* * * * *Reviews of Books* * * * *

General

ANALYTICAL PHILOSOPHY OF HISTORY. By *Arthur C. Danto*. (New York: Cambridge University Press. 1965. Pp. xi, 318. \$10.00.)

PROFESSOR Danto's book is useful to the historian as one of the few comprehensive attempts to approach basic problems in the philosophy of history from the viewpoint of analytical philosophy. Danto's discussion centers around four traditional points of controversy that have also occupied historians in recent years: whether a substantive philosophy of history is possible; the possibility of objective knowledge versus its total subjectivity and relativity; the applicability of general laws similar to those of the natural sciences to historical explanation (Hempel, Popper) as opposed to the inapplicability of such laws (Dray, or, in a different form, Dilthey, Croce, and Collingwood); "methodological socialism" stressing the central role of social forces in historical change in contrast to "methodological individualism" seeking to reduce all societal facts to individual human behavior. Danto rejects outright the possibility of a substantive philosophy of history, but seeks a middle position on the other three sets of alternatives. Our knowledge of the past, he admits, must always be fragmentary because what is significant in the past is determined by later events, including developments still in the future. All history is thus interpretation written from a time-bound perspective. Nevertheless, Danto refuses to draw the radically relativistic conclusion that because the historian does not experience the past directly (Ayer) or see it independently of his "time, place, and personal attitudes" (Beard), he is incapable of seeing it as actuality. The historian, like the painter, is selective. He neither can nor wants to re-create the past perfectly, but seeks to interpret it. The past does not change, but our manner of organizing it does. The difference between history and natural science, Danto believes, does not lie in the historian's use of organizing schemes that go beyond what is given and the scientist's refusal to use them—both use such schemes—but in "the kind of organizing schemes employed by each. History tells stories." The narrative character of history provides the key to the problem of causation and that of the primacy of societal or individual factors. The historian never seeks to explain an individual event; he is concerned with describing the changes that his object of study undergoes. The narrative description itself, Danto argues, constitutes a form of explanation peculiar to historical writing, which indeed makes use of general laws without ever seeking to subsume complex historical realities under one general covering law.

Danto's book should be of interest to historians at a time when they have become increasingly concerned with questions of the theoretical foundations of historical knowledge, which in the past they had largely left to the philosophers and social scientists. The recent publications of the Social Science Research Coun-

cil's Committee on Historiography and the creation of the journal *History and Theory*, in which Danto's key chapter on "Narrative Sentences" first appeared, reflect the beginning of a dialogue between historians and philosophers to which Danto's book is an important contribution.

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VISIONS OF CULTURE: VOLTAIRE; GUIZOT; BURCKHARDT; LAMPRECHT; HUIZINGA; ORTEGA Y GASSET. By Karl J. Weintraub. (Chicago: University of Chicago Press. 1966. Pp. 308. \$7.50.)

THIS interesting book, appropriately coming from the University of Chicago, where "world history" and, therefore, cultural history are still taken seriously, is made up of six loosely connected essays on major theorists and practitioners of culture and cultural history. As the author admits, other historians might have made other choices. Choosing was difficult, for Mr. Weintraub's principle of selection and the thread of his argument lead away from unity into diversity and alternatives. His task was further complicated because of the different problems posed by his subjects. Of the writers studied, three—Burckhardt, Lamprecht, Huizinga—wrote on the systematics of their subject; of all the writers, only Burckhardt and Huizinga produced what are still recognized as model studies in the genre. From Voltaire's journalistic version of *le siècle d'or*, Weintraub has had to extrapolate a view of Western cultural history; from Lamprecht's lucubrations on *Seelenleben*, to condense one. The book is a study of cases in the history of ideas and the history of history rather than a set of schemes for thinking about or writing cultural history.

Cultural history is a problematic topic; professional historians have moved away from synthetic histories, opting to leave "culture" to anthropologists, distrusting folkish, psychological, and metaphysical explanations for "patterns" of history. In our present-minded, analytical age, these historians (even Guizot, the most "presentist" of the lot) get short shrift; indeed, it is difficult not to associate cultural history with conservatism, in spite of the fact that of the six authors dealt with here, most made some gesture defying the conservative barbarisms of their own age. Even notorious believers in progress such as Voltaire and Guizot did not believe that progress was inexorable, and they exhorted their readers to contribute consciously to it. Burckhardt and Huizinga regarded traditional civilization as the only defense against the barbarization of postrevolutionary Europe; Lamprecht's theory was based upon the principle of "das Volk"; Ortega broke off his self-imposed exile to return to Spain. For believers in civilization and culture, there is danger of simplifying civilization and culture to *Bildung*, traditions, manners, continuities, politesses, dismissing (as Burckhardt and Huizinga did) the real dangers of collective life as mere lack of taste. The author has high hopes for a revival of serious cultural history, but he does not discuss the question of impediments, eminently relevant. Perhaps he will do so in another study.

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JUDENTUM: SCHICKSAL, WESEN UND GEGENWART. In two volumes.

Edited by *Franz Böhm* and *Walter Dirks*. With the assistance of *Walter Gottschalk*. (Wiesbaden: Franz Steiner Verlag GmbH. 1965. Pp. xiv, 466; vi, 468-953.)

DESPITE their title, these volumes do not offer a comprehensive treatment of Jewish history and religion. The twenty-three contributors, for the most part, have addressed themselves to specific subjects, neglecting certain large and important areas. The influential Babylonian Jewish community of the Abbasid period and even American and Soviet Jewry receive only passing notice. The focus is upon the German Jews and especially their extraordinary relationship to modern German society, the so-called "German-Jewish symbiosis." Although there are also competent essays on Jews in the ancient and medieval worlds and theologically oriented studies of the Jewish faith, it is only in dealing with the Jewish experience in modern Germany that this work makes fruitful reading even for the specialist.

Four essays are devoted, at least in part, to the religious, scientific, cultural, and economic contributions of German Jewry. One of the surveys—on Jewish scientists—consists of little more than pietistic name-dropping. The others try to relate the quite amazing productivity of German Jewry in some way to its Jewishness. They offer theories that are suggestive, if not always convincing. Alfred Weber attributes the modern "explosion" of Jewish creativity to a traditional passion for social morality combined with the critical potential of a group that gains sudden access to historical traditions it has not itself helped to shape. K. H. Rengstorf finds the cultural productivity of German Jewry uniquely Jewish in the central position it assigns to the human being as human being, a result both of the Biblical tradition of one humanity under God and the heritage of oppression. Ernst Fraenkel successfully outlines the economic functions performed by an occupationally restricted medieval Jewry, but for the modern period is forced to employ such vague and doubtful concepts as the "Jewish spirit of enterprise" and the "eminent individualism of the Jews" in the attempt to isolate a differentiating characteristic.

As might be expected, much attention is devoted to all aspects of German anti-Semitism, though unfortunately no single study evaluates the relative influence of the various causal factors. The late Karl Thieme explains its religious origins; Ernst von Schenk relates it to nationalism. Werner Cahnman concludes that the socioeconomic basis of anti-Semitism is the Jew's suspect position as mediator: between East and West, ruler and people, producer and consumer, artist and audience. The most interesting essay is Peter von Haselberg's "Psychologie des Antisemitismus." Haselberg analyzes in depth the catalogue of rationalizations offered to explain the Nazi atrocities. The persistence of anti-Semitism among German youth who have never seen a Jew, Haselberg argues, points to an origin in aggressive tendencies within the anti-Semite and not in the distorted view of a real object. He also explores the anti-Semite's compulsion to imitate the object of his hate, the fascination with forbidden animality, and the desire to identify with ego-restricting authority.

A final section, devoted to Jews in the present, likewise concentrates on German Jewry: its emigrants, its survivors in Germany and Israel, the reparations paid it by the government, and, once again, the anti-Semitism it has experienced since the war.

Hebrew Union College, Los Angeles

MICHAEL A. MEYER

THE RENAISSANCE IMAGE OF MAN AND THE WORLD. Edited by Bernard O'Kelly. ([Columbus:] Ohio State University Press. 1966. Pp. x, 186. \$6.00.)

SYMPOSIA, on the Renaissance as on other subjects, undoubtedly serve a useful educational purpose, but it is not clear that contributions to them regularly deserve publication as a book. The symposium held at Ohio State University in the fall of 1961, which is belatedly commemorated by this volume, was intended to discuss, in connection with various aspects of the Renaissance, the "image" the age had of itself and its accomplishment, but neither this focus nor the distinction of the contributors has succeeded in avoiding the usual mixed and uneven bag.

The most interesting pieces in this collection are those of H. W. Janson and Edward Lowinsky. The former employs the development of freestanding sculpture in fifteenth-century Florence to illustrate the emergence of a new conception of man, while the latter relates changes in the conception and the techniques of musical composition to the broader transformation of Western culture. Both mediate freshly and admirably between specialized competence and the needs of general history. But Giorgio di Santillana's effort to establish the major and creative significance of Paolo Toscanelli in the thought of the fifteenth century, though suggestive for the general intellectual climate of the age, seems to me forced and unconvincing, and the contributions of P. O. Kristeller on Renaissance philosophy and humanism and of Douglas Bush on literature largely repeat views that have long been familiar in their other writings. Bernard O'Kelly's introduction to the volume is, however, a graceful essay on the problems of historical perspective.

University of California, Berkeley

W. J. BOUWSMA

VERKENNING EN ONDERZOEK: BUNDEL AANGEBODEN AAN DE SCHRIJVER BIJ ZIJN AFTREDEN ALS HOOGLERAAR AAN DE RIJKSUNIVERSITEIT TE GRONINGEN. By P. J. Van Winter. (Groningen: J. B. Wolters. 1965. Pp. 451. 29.50 gl.)

HONORING a scholarly career of forty years and more, this collection of essays by P. J. Van Winter appears as he retires from the University of Groningen. A bibliography of his writings is appended.

Most of the selections deal with Van Winter's major interests (as shown in a pair of two-volume studies): the relations between the Netherlands and the young republic of the United States, and the history of the Boer republics of South Africa. There are episodes from the attempt of Dutch financial interests to play an important role in the developing American economy. Dutch hopes proved illu-

sory; the help given the colonies in their struggle for independence by no means guaranteed a favored position in their economic expansion. Similarly, roughly a century later, the religious kinship between Dutch Calvinists and Paul Kruger's Boers failed to produce lasting results. The recounting of the abortive attempt to build an Afrikaner railway network with Dutch and German capital summarizes Van Winter's extensive investigation of the subject. There is a valuable study of the differences between the earlier Boer settlers of the "near frontier" and those who later trekked far to the north. Reflected even today in Afrikaner political alignments, these differences made united action difficult. The American reader will be constantly driven to make comparisons with his own frontier history (where, for example, an analogue to apartheid is to be found in US Indian policies). Related to the author's special concerns are more general essays: reflections on the "American heritage" (frontier or Puritan?), discussions of mid-nineteenth-century British imperial theoreticians (notably Wakefield and Gray), thoughts on possible connections between population and colonization, and the like. Van Winter has also kept a steady interest in the local history of Groningen and the surrounding area, represented here by an exhaustive study of the province's boundaries. This involves some excursions into the medieval history of the area: the power of the town of Groningen overshadowed that of the rural nobility, and the conventional stereotyped picture of medieval society hardly applies.

In summary, then, this collection honors and reflects the work of a careful craftsman with a wide range of inquiry, and several of the essays are of general interest.

Calvin College

DIRK W. JELLEMA

THE HISTORIAN'S CONTRIBUTION TO ANGLO-AMERICAN MISUNDERSTANDING: REPORT OF A COMMITTEE ON NATIONAL BIAS IN ANGLO-AMERICAN HISTORY TEXTBOOKS. By *Ray Allen Billington* with the collaboration of *C. P. Hill et al.* (New York: Hobbs, Dorman and Company. 1966. Pp. xv, 118. \$3.50.)

COMPETENT writers of history textbooks for secondary-school students attempt the difficult task of blending judiciously the romance of the subject, the precision of scholarship, and the generalization rooted in rational inquiry. What makes this desirable objective well-nigh impossible of attainment is the obsolescent nature of textbooks. With the passage of even a relatively short period of time, the romance may turn out to be legend and myth; accepted fact may prove to be outright fiction; scrupulous scholarship may emerge as incomplete research; and the once tenable generalization may disintegrate under the weight of new interpretations. For these reasons it is obviously quite easy to find fault with history texts.

The present volume adds a new dimension: Anglo-American misunderstanding. This report, an investigation by three American and two British historians into national bias in American and British history textbooks for the secondary schools, deserves the attention of authors, publishers, and teachers. Supported by American and British learned societies in history, this inquiry should be read not for its obiter dicta about the qualifications of history teachers and courses of study

but for its analysis and exposition of bias. Limiting its inquiry to the treatment of the American Revolution, the War of 1812, and World War I, the committee finds all the textbooks guilty of one form or another of national bias. The indictment charges that the treatment of each of these three episodes is punctuated by sins of omission (unconscious falsification, bias by inertia, overcondensation) and commission (fighting words, nuances in language, patriotic prose, cumulative implication, and bias in reverse). The halo of national pride combined with ignorance of the latest scholarship has created a variety of forms of bias which, in the opinion of the committee, undergirds Anglo-American misunderstanding.

The recommendations of the committee are not very imaginative. Exhortation to authors to purge themselves of bias is commendable, but psychologically difficult to achieve. Also, the well-intentioned advice that writers keep up to date with historical scholarship founders on the mass of literature that accumulates each classroom teachers. A textbook written by an Anglo-American team of classroom

More to the point would be periodic reviews of texts by teams of historians and year.

teachers and historians might be even more helpful. Best of all would be a history book for American and English secondary schools written by a multinational team of historians and classroom teachers.

This report, ironically, suffers from a unique bias of its own: duonationalism. Although the committee is sensitive to the anticipated charge of Anglo-American provincialism, it offers an apologia based on the belief that the United States and the United Kingdom "personify the democratic values so essential to maintain a world of clashing ideologies." If international thinking and understanding are as desirable as the committee professes, this type of bias could well be relegated to the limbo of history. The international dimension has a scope wider than the Anglo-American community.

It is to be hoped that this volume will initiate a continuing enterprise designed to upgrade history textbooks in countries concerned with the improvement of international understanding.

Queens College

ISIDORE STARR

EKONOMICHESKIE SVIAZI ROSSII SO SREDNEI AZII: 40-60E GODY XIX VEKA [The Economic Ties of Russia with Central Asia: 1840's to 1860's]. By M. K. Rozhkova. (Moscow: Izdatel'stvo Akademii Nauk SSSR. 1963. Pp. 236.)

RUSSIAN expansion into Central Asia in the nineteenth century certainly was not a *mission civilisatrice*, as some Russians of the time argued; nor were the Russian incursions "purely capitalist colonial conquests," as Liashchenko asserted in the Soviet era, following, of course, the views of Pokrovski. Few Soviet historians would take such an extreme position today, although there is still debate over whether economic factors should take primacy over political ones in explaining why the Russians seized the eastern tip of the Muslim crescent at about the same time that the French were appropriating the other end for perhaps more clearly discernible reasons. Those who follow the views of M. K. Rozhkova in her most

Erratum: The third and fourth paragraphs of the review of *The Historian's Contribution to Anglo-American Misunderstanding: Report of a Committee on National Bias in Anglo-American History Textbooks* by Ray Allen Billington *et al.*, on pages 527-28 of this issue, should read:

The recommendations of the committee are not very imaginative. Exhortation to authors to purge themselves of bias is commendable, but psychologically difficult to achieve. Also, the well-intentioned advice that writers keep up to date with historical scholarship founders on the mass of literature that accumulates each year.

More to the point would be periodic reviews of texts by teams of historians and classroom teachers. A textbook written by an Anglo-American team of classroom teachers and historians might be even more helpful. Best of all would be a history book for American and English secondary schools written by a multinational team of historians and classroom teachers.

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recent monograph would agree with what historians in England and America, notably B. H. Sumner, have said about the subject. Sumner, of course, had only a peripheral interest in the Russian conquest of Central Asia and did not have access to archival materials in the Soviet Union. Rozhkova is an expert in this field, and this work, which may be considered a sequel to her *Economic Policy of the Tsarist Government in the Middle East in the Second Quarter of the Nineteenth Century and the Russian Bourgeoisie* (1949), makes abundant use of Soviet archives, including those in Central Asia.

Her documentation supports the contention, shared by many historians, that although trade between Russia and the Central Asian khanates intensified somewhat in the 1860's, this area did not become a significant market or producer of raw materials for Russian mills until much later. Although the tsarist government was aware of the economic potential of the area, it was far from obligated to the pleas of Russian merchants and industrialists for aid and in fact failed to take decisive measures to develop Central Asia as a colony until the end of the century. The conquest of Central Asia, when the impetuosity of local military commanders could be controlled, was primarily a move in Russia's long-standing struggle with England on several fronts, rather than an objective of Russian capitalists.

The fact that political motives were primary in Russia's advance into Central Asia in the 1860's and 1870's does not, of course, mean that the economic relations between the two areas in the same period are unworthy of study. It is to this subject that most of the chapters of Rozhkova's work are devoted. Her book is important, not as a study of premature economic imperialism, but as consideration of a phase in the early industrialization of Russia, of the emergence and nature of capitalism in that country, a subject about which little is known. Her most important chapters deal with the growth and tempo of trade between Russia and Central Asia in the middle of the nineteenth century, the awakening of interest on the part of Russian industrialists in the area, the activities of trading companies, and transportation problems. Readers may dispense with the ritualistic first chapter and with the curious and largely superfluous digression on Anglo-Russian "rivalry" in Central Asia, which appears later on.

New York University

WILLIAM L. BLACKWELL

LEO XIII AND THE RISE OF SOCIALISM. By *Lillian Parker Wallace*. ([Durham, N. C.:] Duke University Press. 1966. Pp. viii, 464. \$10.00.)

In his encyclical "On Socialism" (*Quod Apostolici Muneris*) of 1878 Leo XIII asked in the words of Paul to the Corinthians, "for what participation hath justice with injustice or what fellowship hath light with darkness?" Professor Lillian Parker Wallace has attempted to answer this query by placing in juxtaposition the pontificate and social thought of Leo XIII and the history of Marx and Marxism. The major phases of Leo's reign are carefully and sympathetically narrated, and the career of Marx and the conduct of his disciples to the end of the nineteenth century are retold in a straightforward manner.

It is to be regretted, however, that Wallace did not more rigorously execute the

difficult comparison she so courageously set for herself. The contemporaneous effort of an ancient church to understand the conditions of modernity and that of a new moral philosophy to convert the world is indeed an exciting historical problem. In the eighteenth century the ideas of the Enlightenment outmatched and overwhelmed an exhausted scholastic system, but in the nineteenth century a reinvigorated scholasticism made the conflict less uneven. Yet the failure of understanding between the two systems continued during their Marxian and Leonine stages. This pioneer work does not illuminate very effectively the fundamental differences between the philosophies. The author's concern to appreciate Leo's achievement leads in fact to a serious misreading of his concessions to modernity. Wallace argues that Leo XIII rejected "the idea of inborn inequality among men from the standpoint of endowment by nature. . . ." It is true that in *Rerum novarum* Leo insisted that the state must treat all citizens as equal, but he also expressly wrote: "all striving against nature is in vain. There naturally exist among mankind manifold differences of the most important kind. . . ." In 1891 Leo compassionately presented the injustices suffered by the workingman, but he did not reject his earlier view of 1878 that the Church in its wisdom recognizes "the inequality of men."

Wallace's bibliography reasonably represents the formidable literature devoted to Marx and Leo XIII. Yet the omission of J. B. Duroselle, *Les débuts du catholicisme sociale en France (1822-1870)*, and Jean Yves Calvez and J. Perrin, *Église et société économique*, is noticeable. And Marx might have been treated in a more sophisticated fashion if the author had consulted Calvez's *La pensée de Karl Marx* and George Lichtheim's *Marxism: An Historical and Critical Study*.

University of Wisconsin

EDWARD T. GARGAN

THE ANGLO-JAPANESE ALLIANCE: THE DIPLOMACY OF TWO ISLAND EMPIRES, 1894-1907. By *Ian H. Nish*. [University of London Historical Studies, Number 18.] (London: University of London, the Athlone Press; distrib. by Oxford University Press, New York. 1966. Pp. x, 420. \$10.10.)

THIS is the most thoroughly researched work on the Anglo-Japanese Alliance that has been published so far, and it is a significant contribution to the literature on international relations. Dr. Nish has utilized extensively the relevant materials in Britain's archives in executing this highly competent volume, and in this respect it may be regarded as a definitive work.

By taking the period 1894-1907 the author has been able to devote ample space to the historical background, namely, the international developments in power politics that led almost inexorably to the alliance. A special merit of the book is the treatment of the internal political conditions and forces at work in the two countries that affected diplomacy, particularly the negotiations for the alliance. No other work follows the step-by-step progress of the negotiations so carefully and in such detail to demonstrate how obstacles had to be surmounted.

The book is divided into three parts. Part I, covering the period 1894-1900, shows how once the Sino-Japanese War exposed China's weakness, European powers, particularly Russia, France, and Germany, descended on the helpless

Celestial Empire and established spheres of influence to threaten not only China's territorial integrity but also Britain's paramount position in the China trade. Britain undertook the occupation of Weihaiwei as a counterpoise following its evacuation by Japanese troops. As a result of participation as allies in the expedition against the Boxers, Britain and Japan were drawn closer together. Part II deals with the South African war which led to the end of the policy of "splendid isolation." With Russia becoming increasingly a threat to both nations and in the face of power politics and intense rivalries in northern China following the Boxer uprising, Britain and Japan began cautiously but positively to negotiate an alliance that was concluded in a matter of months. Part III takes up the Russo-Japanese War which was in effect "underwritten" by the alliance for without it Japan could not have decided to go to war with Russia. The author demonstrates that although Japan's victory changed the balance of power in the Far East and Russia was no longer a threat to either Britain or Japan, the alliance was renewed in 1905, and this led to rapprochement with Russia.

Students of international relations and diplomacy will find this book indispensable for an accurate knowledge of the enormously significant alliance that altered the course of history. This work is a necessity for those who would examine in a new light what started the chain reaction that is still far from over.

Yale University

CHITOSHI YANAGA

KOREA AND MANCHURIA BETWEEN RUSSIA AND JAPAN, 1895-1904: THE OBSERVATIONS OF SIR ERNEST SATOW, BRITISH MINISTER PLENIPOTENTIARY TO JAPAN (1895-1900) AND CHINA (1900-1906). Selected and edited with a historical introduction by *George Alexander Lensen*. (Tallahassee, Fla.: Diplomatic Press. 1966. Pp. 296. \$12.50.)

THIS interesting contribution to diplomatic history is built around pieces of the voluminous and unpublished diaries of Sir Ernest Satow (1843-1929), minister to Japan and to China in the decade between the Sino-Japanese and Russo-Japanese Wars. Lensen, who rightly characterizes this decade as a historical divide in modern Far Eastern international relations, has grouped Satow's Japan excerpts around issues of Japanese-Russian rivalry in Korea and the Liaotung Peninsula, while the China years are organized around the Russo-Chinese convention for the evacuation of Manchuria and preparations for the Russo-Japanese War. To clarify chronology, diary excerpts are filled out by occasional quotations from official dispatches and a sketchy narrative. Inevitably the book is not a completely successful whole, and one wonders whether Satow could not have been allowed to summarize his impressions (particularly of the Russo-Japanese War) more effectively and fully. The volume is, nevertheless, full of interesting and often illuminating views from a diplomat of experience and wisdom, and the editor has earned our gratitude by making these available.

Satow's Japan years are the more interesting. He was on home ground in Japan. He spoke Japanese fluently, his contacts were wide, and he was working with leaders he had known since the 1860's. His views of the first impact of the

triple intervention upon the Meiji leaders are particularly interesting. They did not share or at least show the indignation of Japanese journalists and politicians—indeed he thought them flattered by being treated “as if they belonged to the European concert”—but they became discouraged as Russia took over their gains in Liaotung. “They have the appearance of being thoroughly disheartened,” he wrote, “and they do not seem to appreciate the value of diplomacy, except as a preliminary to the use of force.” Satow tried to prod them out of this attitude, itself curiously prophetic of the years after World War II.

In China, on the other hand, Satow found himself dealing with fellow diplomats in a setting dominated by foreign rivalry. The few Chinese leaders who appear are helpless spectators of the impending war between Russia and Japan. Satow himself emerges as a shrewd and astute observer and participant, optimistic about Japanese intentions, and, while more sensitive to East Asian views than most of his contemporaries, still representative of the Europe-centered diplomacy of his day.

Princeton University

MARIUS B. JANSEN

BRESTSKII MIR [The Peace of Brest-Litovsk]. By *A. O. Chubar'ian*. (Moscow: Izdatel'stvo "Nauka." 1964. Pp. 245.)

THIS is a dreadful book. It is also an apparently needless one, for it covers the same period and most of the same issues treated in S. M. Maiorov, *Bor'ba Sovetskoi Rossii za Vykhod iz Imperialisticheskoi Voiny* [The Struggle of Soviet Russia to Withdraw from the Imperialistic War] (1959). Many of the sources and archives used are identical. The interpretations and conclusions are also very similar. If anything, Chubar'ian's account is even more distorted than that of Maiorov in regard to the “treacherous” and “anti-Leninist” actions of Trotsky, who is blamed for the presence of representatives of the Ukrainian Rada at the Brest talks and who is accused of deliberately disobeying Lenin in advancing the “no war, no peace” formula at the conference. Chubar'ian is also more anti-American than Maiorov, charging that the United States attempted to block the peace, to organize intervention in Russia, and to turn the former tsarist empire into a colony of American “imperialism.”

Though repetitious and one-sided in its historical content, the book does serve an obvious political purpose. Chubar'ian strongly defends the principle of “peaceful coexistence” and castigates the “Left Communists” of 1918, maintaining that their position was not only mistaken but extremely dangerous. He equates them with present-day “dogmatists” (that is, Chinese) and concludes that the history of the Brest-Litovsk peace shows how wrong the latter are. The author also takes great pains to “prove” that the Bolshevik quest for peace reflected a fundamental tenet of Soviet foreign policy, not a tactic or propaganda device as Kennan and other “bourgeois” authors aver.

Deliberately excluding any extensive consideration of German policy and Soviet-German relations, Chubar'ian devotes well over half of the book to Soviet-Western relations between November 1917 and mid-March 1918. Archival ma-

terials, used sparingly, contribute little, except occasionally interesting detail. The most significant and useful part of the study is a brief section, with some selective documentation, on the widespread opposition to Lenin's peace policy in party and other organizations outside Petrograd.

A perverted, doctrinaire work such as this is doubly disappointing in view of the quite good scholarly studies Soviet historiography has occasionally produced since 1956. But apparently on sensitive issues of Soviet foreign policy and Soviet-American relations, history is still too close to politics, and the heavy hand of Stalinist anti-Americanism cannot yet be shaken off.

Indiana University

JOHN M. THOMPSON

HISTOIRE DE LA GUERRE FROIDE. Volume I, DE LA RÉVOLUTION D'OCTOBRE À LA GUERRE DE CORÉE, 1917-1950. By *André Fontaine*. [Les grandes études contemporaines.] ([Paris:] Fayard. 1965. Pp. 501. 24 fr.)

THIS objective and well-written survey of East-West relations covers a much more extended period than the author originally intended. He had planned to begin his account with the breakdown of the wartime Big Three coalition, but he soon became convinced that to explain the resulting antagonisms it was necessary to go back to earlier events and to assess their effect in forming the attitudes and the preconceptions that hardened into the enmities of the cold war.

Consequently, two-thirds of the volume is devoted to the years 1917-1945. Much of this well-trodden ground will be familiar to any informed reader. Yet the summary serves a useful purpose in establishing, and from time to time illuminating, the character of the relations that evolved during these years. M. Fontaine is skillful in tracing the pragmatic adaptations of Soviet policy, from the desperate search for peace at Brest-Litovsk through Rapallo to the era of the popular front. He is sound on the implications of Munich, and he gives an excellent brief account of the maneuvers that issued in the Nazi-Soviet Pact.

It was during the uneasy wartime alliance that the basic issues began to emerge with increasing clarity; these are discussed with shrewdness and perception. The author emphasizes the lessons with regard to future security that the Russians drew from Hitler's invasion and their determination to secure not merely their gains under the Nazi-Soviet Pact but the further political and territorial advantages Hitler had refused in 1940. The Kremlin had its picture of the postwar balance of power constantly in view, and the determined pursuit of maximum claims based on military success brought a mounting mutual distrust whose roots are perceptively examined in these chapters.

The second part of the volume is devoted to the five years following the end of hostilities. The panorama is drawn on a global scale, with a fine sense of balance and with many illuminating illustrations. The author's objective approach does not exclude some neat touches of irony, nor prevent him from drawing a picture of Stalinism in action that is as depressing as it is condemnatory. In this phase, when there are so many issues erupting and interacting in kaleidoscopic fashion, the author's perspective is on the whole admirable. There may be differ-

ences of opinion as to which events deserve fuller treatment, and it may also be felt that the documentation is curiously selective. For a book of this scope, however, there are many alternative sources, including the author's extensive personal experience. The volume as a whole is a commendably successful effort to bring into focus the origins and nature of the conflicts that so disturbed and divided the immediate postwar world.

York University

EDGAR MCINNIS

BEGINNINGS OF THE COLD WAR. By *Martin F. Herz*. [Indiana University International Studies.] (Bloomington: Indiana University Press. 1966. Pp. x, 214. \$4.95.)

THIS short volume will interest undergraduates seeking to understand the confusing diplomacy that marked the end of the Second World War in Europe. The author has used well-known printed works—Cordell Hull's *Memoirs*, the *Yalta Papers*—and in clear, straightforward prose analyzed the origins of the so-called cold war. He stresses the issue of Poland, with some discussion of other East European countries, including a short look at American financial policy toward the Soviet Union. The book concludes with questions and answers for the student and retrospective comments of such public figures and authors as Averell Harriman and Philip E. Mosely. The book will remind its readers that the Russians viewed the postwar settlement narrowly in terms of borders and friendly governments, with enormous memory of history, especially grievances against the Poles.

If this able, intelligent, and (within its limits) even brilliant volume has a flaw, it is its failure, paradoxically, to take positions beyond the available evidence, however unscholarly such a suggestion may seem. For the most important single factor in the deterioration of American-Soviet relations may well have been the Russian dictator, a peculiar individual, whom one distinguished American expert on Soviet affairs has described as possessing a "personality disorder." In the origins of the cold war the imponderables may have governed. Perhaps Stalin's inscrutable suspicions made all the *pourparlers* essentially meaningless.

Indiana University

ROBERT H. FERRELL

Ancient

LA PRÉHISTOIRE. By *André Leroi-Gourhan et al.* ["Nouvelle Clio": L'histoire et ses problèmes, Number 1.] (Paris: Presses Universitaires de France. 1966. Pp. 366. 22 fr.)

PREHISTORIANS are at last beginning to show concern for the preliterate culture history of the world as a whole. It is probably fair to select Grahame Clark's *World Prehistory—An Outline* (1961) as the pacesetter. With this book we have a similar and worthy such first attempt by a competent group of French col-

leagues. In my opinion, this would also be a valuable book for students in English.

The book is both well planned and interesting. Book I ("Sources") includes a résumé by geographical region and general chronological order of the world's major prehistoric sites; a good working bibliography is appended. In Book II ("Our Understandings") J. Chavaillon treats the lower Paleolithic age; A. Leroi-Gourhan, the middle and upper Paleolithic age of the Old World; A. Laming-Emperaire, the Mesolithic and Australia and Oceania; G. Bailloud, the Neolithic; and Laming-Emperaire and C. Baudez together, the New World. And in Book III ("Problems and Directions of Research") the "problems" considered are grouped as methodological, chronological, and ethnological. H. Balfet, M. Brezillon, and Leroi-Gourhan join the other authors in considering them. The number, variety, and quality of the line illustrations, especially of types of flint tools, are highly useful.

The book is provocative in the attention it gives to the swing from food gathering to food production, although it seems curious that the work of one of the most imaginative and fruitful French scholars in the field, Jean Perrot, receives little if any notice. There is much less concern with the (still prehistoric) problems of the threshold to urban civilization. I long ago gave up the struggle to get precise meanings out of the learned-sounding neogracisms, "Mesolithic," "Neolithic," and so forth, and I was amused to watch the continued struggle. For Laming-Emperaire, "les vrais Mesolithiques" were the inventors of food production. For Bailloud, it would appear that "sociétés Proto-néolithiques" were those on the road to "néolithisation" (that is, food production). But nomenclature is a sticky problem, given the ambiguities of prehistory, and this remains a commendable book.

University of Chicago

ROBERT J. BRAIDWOOD

A HISTORY OF GREEK LITERATURE. By *Albin Lesky*. Translated by *James Willis* and *Cornelius de Heer*. (New York: Thomas Y. Crowell Company. 1966. Pp. xviii, 921. \$15.00.)

"Behind all the ingenious subjectivism [of some current classical studies] . . . one detects a shrinking from honest discussion and a contraction from real knowledge. . . . Our age has become too lazy in its attitude to [literary] history. . . . I had no intention of sparing detail." In his text the facile, shallow, brilliantly dull books are dismissed one after the other. To discourage such works is one of Lesky's avowed objects.

But the negative aspect is minor. About each classical author Lesky attempts to determine what really is known; the literary tradition behind his writings; the manuscripts; the content of the writings, with some criticism of them, unstrained, often merely implicit; and abundant bibliography (into 1961, some in 1962), in addition to much in the footnotes. What is interesting and important? What precisely is the controlling evidence? What conclusions can fairly be drawn? These are questions the author set himself to answer. The book is therefore not just a

summary of established facts; it is also a series of exciting inquiries. Lesky has wonderful generosity of interest, and his energy is titanic. The interest is reflected in the style, which the translators of the second edition of *Geschichte der griechischen Literatur* (1st ed. issued in pts., 1957-58; 2d ed. in 1 vol., contents much expanded, 1963) have kept highly readable, and the energy is reflected in the contents. Beginning with ninety pages on Homerica, and ending with ninety pages on the Roman Empire (down to about A.D. 600), the book is inclusive and well proportioned. Not unduly technical, and moreover cheap, with nine hundred large, luminous pages, it may well find a place in any educated person's library.

Such is Lesky's work, the world's leading history of Greek literature. Most of the judgments appear to be sensible; exceptions, which are rare, are perhaps due to old doctrines not outgrown. L. H. Jeffery, *Local Scripts of Archaic Greece* (1961, but evidently too late), could have saved him at the outset from adherence to a date of about 900-850, at least a century too early, for the introduction of (Phoenician) writing. "We share with most scholars the view that the composition of the two [Homeric] epics presupposes writing"; Homeric poetry, though oral in technique, was *written* by Homer. Failure to comprehend the fundamentals of what M. Parry discovered, and A. B. Lord and J. A. Notopoulos have developed, is alarming in the author of the forthcoming article on Homeros in Pauly-Wissowa, *Realencyclopaedie*.

In fields less familiar to him, Lesky is perhaps more open to new views. For him, however, the trial and defense of Socrates took place much as Plato's *Apology* would have us believe; to stress the two mentions in Plato of philosophers silent at their trials is downright opprobrious. The treatment and estimate of Xenophon suffer a little from conventionality, and the treatment of Aristotle's *Constitution of Athens* is not sharp throughout. Even Lesky's energy and interest have limits.

Harvard University

STERLING DOW

EARLY ROME AND THE LATINS. By *A. Alföldi*. [Jerome Lectures, Seventh Series.] (Ann Arbor: University of Michigan Press. [1965.] Pp. xxi, 433, 25 plates. \$15.00.)

THE early history of Rome is traditionally recorded by writers of the Augustan period: Livy and Dionysius of Halicarnassus. Behind them, stretching back to the third century B.C., lay a series of writers in Greek and Latin whose works have been largely or completely lost. At the fountainhead were the Roman aristocrat, Fabius Pictor, who wrote during or after the Second Punic War, and the Sicilian exile, Timaeus, who wrote in Athens two generations earlier. It is disputed whether there were *fortes* before these Agamemnons, but references to Rome, the Latins, and the Etruscans occur as far back as Hesiod. The problem is, then, how reliable are the narratives of Livy and Dionysius concerning the seven kings and the first century of the Republic? A rare few have accepted everything; many have doubted everything. The learned author of this notable volume claims to adopt a middle position, but he is closer to the second than to the first. In effect, he argues that most of the early history was codified and much of it invented by

Pictor, partly (for he wrote in Greek about 210 B.C.) so that the Greek world should know Rome's greatness and not be misled by the victories of Hannibal; and partly for internal reasons, to magnify the achievements of earlier Fabii and to smirch the earlier Claudii, their political rivals. So successful was he that all subsequent writers copied him.

Put in this simple way, the claim tends to provoke skepticism. If Pictor wrote partisan propaganda, why was he believed and accepted in a period of great cultural advance and of intense political activity when, in fact, the Fabii were losing ground against the Claudii and others, notably the Cornelii? But the great value of the author's study lies in his intense analysis of sources outside the traditional narrative, archaeological and institutional. Much has been learned from excavations, and much information exists in later references to cults and civic practices, their origins and forms. The bibliography is extensive, and the author faithfully lays it out for the reader, supporting or opposing his own views. While there is much of the subjective in his conclusions, nevertheless he makes out a persuasive case for his thesis that Etruscan cities, in succession, dominated Rome during the sixth and into the fifth centuries, and that Rome itself attained no political importance until the period of expansion, interrupted only by the Gallic siege, which began after 450.

There are difficulties. Can we derive the three tribes, thirty *curiae*, and three hundred senators from a supposed three hundred mounted guardsmen of the last Etruscan king, who dubbed themselves patricians after the king was ousted? These numbers seem sophisticated, like the number seven in the king-lists. Were the Claudii (or some of them) original patrician Romans and not immigrant Sabines, although the Claudian tribe was not established until the late fifth century? On the other hand, the idea that the Aeneas cult, and so the story of the Trojan refugees in Italy, was brought to Lavinium by the Etruscans in the sixth century seems very apt historically as well as fitting the evidence. There are many such proposals in the volume.

Professor Alföldi is to be congratulated, and we shall study him seriously. It is an exhaustive and important book.

Yale University

C. BRADFORD WELLES

HANNIBAL'S LEGACY: THE HANNIBALIC WAR'S EFFECTS ON ROMAN LIFE. Volume I, ROME AND HER NEIGHBOURS BEFORE HANNIBAL'S ENTRY; Volume II, ROME AND HER NEIGHBOURS AFTER HANNIBAL'S EXIT. By *Arnold J. Toynbee*. (New York: Oxford University Press. 1965. Pp. xii, 643; x, 752. \$40.35 the set.)

A STORY told by Cicero and Livy but emanating from Hannibal's historian Silenus records that Hannibal, when setting out from Spain, dreamed that a destructive monster was following him and learned that it was the fated devastation of Italy. In a sense, that monster is the chief subject of this work. These two stout volumes are not a history of the Punic or the eastern wars, but a study of the situation when they began in 264 and after Carthage submitted in 201. In the first volume a survey of the Greek powers and Carthage leads to a detailed description of the

growth and organization of the Roman "Commonwealth" in Italy, a rewarding study with much appreciation of Roman liberality and successes at this stage. But in Rome itself such weaknesses as the frustration of democratic development and the rise of an "Establishment" receive due emphasis. The second is devoted to the effects of the wars, and traces, as in the scenes of a tragedy, the devastation of southern Italy, the new agriculture, the ruin of the peasantry and decline of the army, the rigid and tyrannical treatment of allies in Italy and outside, the new business classes, and, in general, the almost catastrophic economic, social, and religious changes that ensued. This synthesis is presented with the massive learning, command of specialized studies, independent judgment, long views, ready analogies, and unhurried pace characteristic of the author, and it will remain valuable, whether one agrees fully with his thesis or not.

The thesis, sketched briefly some years ago, is here expanded and sustained. By accepting the Campanian alliance (about 340) Rome condemned itself to a choice between being ruined by hostile forces or expanding first up to the bounds of peninsular Italy, and ultimately to the future boundaries of the Empire. Entrance into the Greek world forced it to come to terms at some point with such major institutions as monarchy, professional armies, capitalistic farming, and the formation, as in Ptolemaic Egypt, of a civil service. The double war with Carthage, the devastation of southern Italy, and the eastern wars that followed overturned the international balance of power and speeded the process of adjustment to revolutionary proportions, forcing it irreversibly into unsound lines, while governing class and citizen body degenerated. The eventual result was the century of revolution that began with the Gracchi, ruinous for Italy and the Greek world alike, so that Augustus was able, at best, to win only a postponement of the fate of a society already infected with the seeds of decay. Granting that this is a valid outline description of what happened under the Republic, one may doubt the necessity of the various steps and reflect on the irony that measures, which must often have seemed wise and proper choices to their time, had unforeseen results. The tradition of the Campanian alliance is far from clear, and the Samnites were unruly and aggressive neighbors. The Roman government hesitated before entering Sicily. Another than Hannibal might not have succeeded in entering Italy at all. Division in the Greek world and Philip's own aggressions were factors in causing the Second Macedonian War, and the course of the negotiations shows that both Rome and Antiochus avoided hostilities for years. Parts of Lucania and Apulia were always better suited to grazing, and the decline of Magna Graecia began in the century before the Punic Wars. The devastation of the Hannibalic War still left the land available and capable of restoration. Shortage of manpower and the strategic needs of Cisalpine Gaul might well have made the large ranches appear to be the best use in that generation. There was some recovery in population and prosperity in Magna Graecia during the Empire, as Kahrstedt's study shows. Security in Cisalpine Gaul and Liguria was surely a pressing problem, yet the new kind of colonies of Roman citizens in Cispadane Gaul may well, as the author holds, show that the needs of the Roman peasant farmers were being considered too. The Roman equites, the new business class, should probably not be distinguished so firmly from the Senate, which

continually drew new recruits from them, though few families rose soon to a leading position. Even in the Greek world divisions had weakened the defensive strength of their major institutions. With Augustus, Rome made a major adjustment to them. Was the Roman Empire so "short-lived"? More than two centuries is a considerable period. And more might be said of the new strength the Empire drew from the provinces. One may question if Rome should be put so firmly into the Greek world from the start, or Latin literature, with its different tone and emphasis, be considered so completely "Greek-style," even in its most imitative period. This review cannot conclude without expressing appreciation for the many vivid descriptions of Italy, the fruit of the author's own travels and observations.

University of North Carolina

T. ROBERT S. BROUGHTON

EMIGRATION FROM ITALY IN THE REPUBLICAN AGE OF ROME.

By A. J. N. Wilson. (New York: Barnes and Noble. 1966. Pp. xiv, 208. \$7.50.)

WHAT induced Romans, Italians, Italo-Greeks to leave the peninsula and settle all about the ancient world? What did they turn to in their new homes? How did they fare? In 1919 Jean Hatzfeld's masterly *Les trafiquants italiens dans l'orient hellénique* told of those who went eastward. A. J. N. Wilson has set himself the task of telling the whole tale.

His book is arranged in two parts: Part I sketches the westward movements; Part II re-presents, with some adjustments, the picture Hatzfeld had drawn. The two come out curiously unlike, largely because of the sources of our information. The West, known chiefly through remarks dropped by the historians of Rome's wars, gives us a story of soldier settlements, of the exiled scurrying for refuge, of the dispossessed seeking new lands to farm. In the East, known through honorary inscriptions, tombstones, and similar sources, or through Cicero's speeches and letters, the lead roles are played by businessmen and bankers, and the themes are the sadly familiar ones of exploitation, profit, and political chicanery.

The subject of Roman emigration has long needed a full-scale study that would exhaust every available form of evidence. This need is still to be met; Wilson provides only a review of the main lines of the movements. It is useful in many respects, particularly for supplying the first ordered account of westward migration, but disappointing in many others. He has utterly ignored all that archaeology can reveal and has not even brought the epigraphical material up to date (a fact obscured by an exasperating practice of omitting publication dates). More serious, his narrative has little breadth or depth; it nowhere seeks to lay bare the interweaving of the strands of politics, economics, and sociology that run through his topic, strands that the historian must trace no matter how miscellaneous and recalcitrant his sources. A chapter promisingly entitled "The Relations of the Settlers with the Local Inhabitants" turns out to be just another account of the Republic's mishandling of its eastern provinces, told, moreover, without the slightest linking up with what, for example, a Verres was doing in the West. In fact, Wilson's sections on West and East bear so little interconnection they could almost have been published as separate monographs.

This is a moderately useful book as far as it goes, but one that hardly goes far—or deep—enough.

New York University

LIONEL CASSON

THE CIVITAS CAPITALS OF ROMAN BRITAIN: PAPERS GIVEN AT A CONFERENCE HELD AT THE UNIVERSITY OF LEICESTER, 13-15 DECEMBER 1963. Edited by *J. S. Wachter*. ([Leicester:] Leicester University Press. 1966. Pp. 124. 25s.)

PERIODICALLY archaeologists and historians of Roman Britain convene to assess new finds, to offer tentative syntheses of accumulated evidence, and to suggest areas for future investigation. This book, the report of a conference on Romano-British cantonal capitals, confirms the value of such conventions and contains, despite an inevitable unevenness in quality, a number of papers that should interest the specialist and nonspecialist alike.

The archaeologist's constant attempt to perfect traditional excavation procedures and his eagerness to adapt new scientific techniques are documented in two interesting methodological papers. Of greater interest to the general reader will be J. K. St. Joseph's assessment—with plates produced by the Cambridge University Committee for Aerial Photography—of the significant contribution of aerial photography to archaeological research since World War II. Illustrative of the great potential of this scientific aid is Silchester, a site systematically excavated early in this century, where the number of new buildings and defense structures detected by aerial survey is remarkable. The less spectacular methods employed and the multifarious problems encountered in dating town buildings and defense structures are concisely surveyed in the complementary paper by B. R. Hartley.

Of the remaining papers three are largely archaeological in character and somewhat limited in scope. G. Webster's examination of the relationship between Roman forts and civilian settlements, Lady Aileen Fox's related paper on Exeter, and Joyce M. Reynolds' discussion of the legal status of tribal capitals and the character of tribal institutions are specialized studies that collectively demonstrate the insecurity and insufficiency of Romano-British data. Less technical is the paper by the late Sir Ian Richmond, to whom this volume is appropriately dedicated, which assesses part of the archaeological evidence of industry in Roman Britain and provides a brief estimate of Romano-British technology. The most ambitious paper is S. S. Frere's "The End of Towns in Roman Britain." Here the literary tradition of a sudden and disastrous sack of Romano-British towns by the Saxons is checked against the archaeological record. Frere, of course, does not resolve this perennial problem. His own view is that the Saxon "conquest" was effected gradually and that many British towns, probably with the assistance of German auxiliary troops, preserved their Romanitas into the fifth century. Related methodologically is the editor's contribution, an examination of second-century town fortifications (hastily constructed, in Wachter's judgment, to withstand the invasion from the north in A.D. 196-197). A. L. Rivet's "Summing-up" completes

the collection and complements the introductory statement by Richmond.

My only criticism of this collection is that cultural history and the process of Romanization are not given greater space, but, of course, a conference can rarely be comprehensive. The prefatory bibliography, complete to 1964, covering the fifteen cantonal capitals is a first-rate guide for further research.

University of Michigan

JOHN W. EADIE

TEMPLES IN ROMAN BRITAIN. By *M. J. T. Lewis*. [Cambridge Classical Studies.] (New York: Cambridge University Press. 1966. Pp. xvi, 218. \$9.50.)

HERE is gathered and synthesized all available archaeological material on "enclosed places of public worship" in Britain during the period of the Roman Empire. Since most monumental evidence is no longer visible, the author has worked from reports of excavations and finds, both written and oral.

The material is divided among four categories: Romano-Celtic temples; classical and simple apsidal, rectangular, or circular temples; Oriental cult places; and temples attested by inscriptions, texts, or minor finds but with no structural remains. Discussion of the Romano-Celtic temples is most significant. The forty-five temples of this type in Britain (concentrated in the southeast) are described and analyzed in plan, materials, details of superstructure in relation to each other and to similar temples on the Continent. Of three possible types of superstructure on the square within square foundations, the tower type in which the central high cella, often with clerestory, was surrounded by a lower-roofed portico, open or closed, was the most common. Study of possible origins offers the conclusion that it developed under Roman influence from adding a portico to an existing square shrine, a conflation of Roman architectural style and building methods with Celtic open-air enclosure, probably first in central or eastern Gaul in early first century B.C. and brought to Britain about 50 B.C. by Belgic immigrants. The amount of elaboration depended on the degree of Romanization of the area. There were no chronological developments (in proportions or otherwise) within the first four centuries A.D. in which these temples were constructed to serve Celtic deities, sometimes on pre-Roman sites. Evidence for chronology is carefully listed; charts group the temples by dates and by location in town, country, or military encampment.

Classical temples (here one feels the lack of *comparanda*) occurred only in strongly Romanized centers, and not before the second century. Mithraea and other Oriental temples, chiefly of the second and third centuries, were located in military areas. Christian churches came into towns in the third to the fifth century, but did not drive out paganism, especially in the country (witness the mid-fourth- to early fifth-century pagan revival). Pagan and Christian structures alike gradually decayed, creating the unusual historical phenomenon of no continuity of sacred spots into Saxon times.

Plans drawn to uniform scale, frequent charts, and lists are welcome aids to this thorough compendium which offers several new interpretations of individual temples as well as thoughtful general historical implications.

Institute for Advanced Study

LUCY SHOE MERITT

Medieval

ZAPADNOEVROPEISKAIA SREDNEVEKOVAIA ISTORIOGRAFIIA
[Western European Medieval Historiography]. By O. L. Vainshtein. (Moscow: Izdatel'stvo "Nauka." 1964. Pp. 482.)

O. L. VAINSHTEIN's book comes armed with the virtues all professional Soviet scholars show, principally technical fortitude, exacting preparation, and thoroughness. Vainshtein moves freely in the whole body of sources for the medieval West, and in fact gives us a reference work, a *Summa Sovietica*, of orthodox Soviet views of individual historians or historical-minded thinkers of the medieval era and its bracketing periods. Of equal or more value is his precise exposition of the accepted canons of Marxist historiography. Vainshtein knows, and pities, the modern Western historians who probe into and agonize over historical patterns, the "truth" in, or of, history, the problem of objectivity. History holds no terrors for the Soviet historian, and therefore he can dispassionately trace the development of processes through which its reality was and is revealed. The Middle Ages made up a period defined by its economic bases, and its historiography (that is, of its elites) is scientifically predictable. So, too, is the historiography of the succeeding capitalist economy in its early stages; it remains only for the author to set the whole in focus. This he can do because "historical science [is a] system of the knowledge of certain facts related to the development of human society," and Vainshtein has these facts.

Medieval historiography thus advances beyond earlier stages principally because it discovered and developed periodization: the linear movement at whose end stands the Marxist historian himself. Vainshtein need neither censure nor forgive its sins: ignorance of facts, "asceticism," falsification, providentialism—all sufficiently remarked by the critics of the humanist reaction and by the reformers of all types. The historical thinking of the latter, in their turn, is pungently characterized. Vainshtein's treatment of individuals goes beyond incantation, however (as, that St. Augustine was "reactionary," but, to give another example, Liutprand of Cremona is "the most brilliant and the most subjective" of his kind). He goes on to the fullest examination possible of the individual tesserae in the patterns he knows he will find. No medievalist can afford to treat this book as a curiosity.

University of Rochester

D. A. MILLER

THE VISION OF HISTORY IN EARLY BRITAIN: FROM GILDAS TO GEOFFREY OF MONMOUTH. By Robert W. Hanning. (New York: Columbia University Press. 1966. Pp. x, 271. \$7.50.)

Just as good wine needs no bush, a good book needs no blurbs, and, indeed, like a poor one, is better off without them. No reviewer can serve two masters. It is difficult enough to assess a book without having to reckon with another man's estimate—short, decisive, eulogistic. Mr. Hanning has written an informative book about four accounts of the fall of Britain—the *De excidio et conquestu* of Gildas, the *Historia ecclesiastica* of Bede, the composite *Historia Brittonum*, and

the *Historia regum* of Geoffrey of Monmouth—against the background of medieval historical imagination. If, as Macaulay put it, “this work will be less acceptable to those who read for the sake of reading, than to those who read in order to write,” the defect by no means cancels the virtue.

In his opening chapter the author, after a bow to classical historiography, concentrates on Eusebius, the welder of the Pax Romana and the Christian order, and does so convincingly. This contemporary of Constantine saw Rome as the fulfillment of many Old Testament prophecies, and his *History* was an attempt to establish the unity of mankind with Constantine as the new Abraham. Although historiographers have often made Augustine the fountain of medieval historical thought, Hanning awards the palm to Eusebius, the more so in that Orosius, the disciple of Augustine, was convinced that the City of God and the City of Man would eventually unite. In examining the British histories Hanning tells us much more than others have recited. Although three-quarters of Gildas is a querulous book of complaints, the other quarter is the source for later historians of early Britain, not only for information but for point of view, a point of view distinctly Eusebian. Bede, writing in a day of peace and triumph as opposed to chaos and defeat, has nonetheless much in common with Gildas: neither writes his account exclusively as a chapter in the history of salvation. The same is true of the *Historia Brittonum*. With Geoffrey of Monmouth we come to a history largely free of Christian assumptions. The complexity of human life has eroded the emphasis on salvation; that the individual has recovered his importance and concern with treachery and lust, with tension between personal and social interests, recalls the classical outlook. Geoffrey, once considered simply as the source of the Arthurian legend, is here given a place at least equal to the greatest names in a most vital historiographical era, and for the best of reasons, as the autobiographer of his time.

University of Missouri

CHARLES F. MULLETT

ABHANDLUNGEN UND UNTERSUCHUNGEN ZUR GESCHICHTE
DES KAISERGEDANKENS IM MITTELALTER. By *Edmund E. Stengel*.
(Köln Graz: Böhlau Verlag. 1965. Pp. 342. DM 44.)

THIS volume of essays and studies in the history of the imperial idea is a significant piece of work because it examines in detail the evolution of *imperium* as a concept in medieval political and constitutional history. The interpretation is centered around the union of the Roman *imperium* with the German *regnum*, achieved under Otto the Great and established more solidly in the twelfth century so that in a certain sense the Empire (*imperium*) is embedded within the German kingdom (*regnum*), and the choice of the king by the German princes is deemed binding on the Empire for the princes may be considered a substitute for that universal people, the *populus romanus*.

The book is divided into five sections. The first finds the basis of imperial authority in military power and develops the thesis *exercitus facit imperatorem*. It traces the idea of the *Heerkaiser* from the emperors of Roman antiquity through the *imperium christianum* of the Carolingian Empire and the “might idea” of

the medieval *Kaisertum*, considered as the incarnation of power, to the matured imperial idea of Otto the Great and the Hohenstaufens.

The second section correlates the concepts of *regnum* and *imperium* representing limited and general jurisdiction. The treatment is chronological although the subject matter is topical: Frankish *imperium*, Roman *imperium* (Germany, Italy, Burgundy), the imperial idea on the northern and eastern marches, and the narrowing range of the concept with the dissolution of the Empire.

The third section treats the problems arising from the German knightly orders that relate to *imperium*, *regnum*, *sacerdotium*, and feudal law. Also it examines the role of the Grand Master, whether prince or prelate. The fourth section is an interesting study of the early history of the modern idea of the state, the relation of the imperial title to the idea of sovereignty, and the new significance of the imperial title independent of Rome. The concluding section is a philological and conceptual analysis of the terms *imperator* and *imperium* in Anglo-Saxon sources: Bede, Adamnan, Alcuin. In general this book breaks new ground and treats the subject more comprehensively than has been done heretofore.

Rice University

FLOYD SEYWARD LEAR

LES INVASIONS: LE SECOND ASSAUT CONTRE L'EUROPE CHRÉTIENNE VII^e-XI^e SIÈCLES). By *Lucien Musset*. ["Nouvelle Clio": L'histoire et ses problèmes, Number 12^{bis}.] (Paris: Presses Universitaires de France. 1965. Pp. 297. 20 fr.)

FOLLOWING the format of the series in which it appears, Musset's book is divided into three portions: it opens with a bibliography and a chronological chart, continues with a narrative account of "what we know" about the post-Germanic invaders of Europe, and ends with the author's "directions of research" in which he discusses the impact of the invaders, the Vikings in particular, upon the lands they struck.

Obviously the author accepted a staggering assignment, and he has completed it as successfully as a single and unaided historian could. Where language barriers prevented him from consulting secondary literature, most notably in the Slavic languages, he has made intelligent use of whatever was accessible to him. He comes into his own when he deals with the Vikings and their impact on Normandy and the Danelaw.

Unfortunately, this recommendation cannot be extended to the portions of the book devoted to Eastern Europe and the Saracens. Even though admitting the necessary limitation of the bibliography to materials in Western languages, the reader will find it erratic and incomplete. And should a historian who is unacquainted with most of the secondary literature in a field venture to propose directions of research?

It is difficult to rationalize the principles of inclusion and exclusion followed by the author in dealing with his immense topic. He excludes, for example, the Muslim invasions of Sicily and Spain because the book is concerned only with those assaults on Christian Europe that did not alter its faith. According to this reasoning, successful wars by infidels are out; unsuccessful attacks by infidels are

in, but so, too, are successful invasions by pagans wise enough to convert before the chronological limits of the eleventh century. The Khazar converts to Judaism are a problem, but they are in, possibly because the rules do not apply to nomads from the steppes. Thus the reader seeking a full account of the Muslim conquest of Spain will be disappointed; he will, however, find a few pages on the *razzias* conducted by Saracens in Italy and Provence.

Essentially, the book lacks real or even thematic unity. As Musset admits, the invasions were uncoordinated, and there was little relation among them until the latter part of the ninth century. At that point the weakness of the Carolingian Empire invited invaders to occupy and destroy it in concert. It is unfortunate that Musset did not find a collaborator to develop the contrasting theme: how Byzantium's strength enabled it to ward off its invaders. Then we might have had chapters on Eastern Europe to match the quality of what Musset has written on the Vikings and the West.

Mount Holyoke College

JOHN L. TEALL

ORIGINS OF THE COMMON LAW. By *Arthur R. Hogue*. (Bloomington: Indiana University Press. 1966. Pp. xii, 276. \$6.50.)

THE NORMAN CONQUEST AND THE COMMON LAW. By *George W. Keeton*. (New York: Barnes and Noble. 1966. Pp. 238. \$6.00.)

HERE are two good books for today's forgotten man: the beginning student. Hogue, a historian, modestly has written "a book which does not assume professional legal training" and "which anticipates the difficulties one encounters when approaching the common law—possibly for the first time." Similarly, Keeton, an eminent London legal scholar, aimed "to explain (as far as possible in non-technical terms) how the Norman Conquest affected the legal system which had been developed in Saxon England, and the extent to which it influenced the subsequent development of the Common Law in England." Immediately, the Conquest made few direct additions to the law, and perhaps the most important were the Norman concept of the fief, which has determined much of England's land law, and the "new legal conception" of felony. Keeton also describes the modification of the language of the law and the law merchant, and the introduction of forest laws, and, if he is correct, of the practice of "balanced government." Keeton feels, nevertheless, that "in some respects the completion of the Danish conquest of England by Canute had more important consequences than the" Norman, and he adds, "certainly Canute was a greater lawgiver and innovator than William." Other arresting generalizations also seem plausible, and the few factual slips may not matter much. To translate *nolunt leges Anglie mutare* as "we will not change the common law of England" is pardonable, but to assign Hengham's reply to counsel in 1305 to his successor, Bereford, and Bereford himself, who died in 1326, to Edward III's reign, is less forgivable.

Hogue's account of the common law may seem elementary, but, based on the best authorities, it is in itself scholarly. Fortunately, he is not too proud to define the legal terms he uses in "A Glossary for Laymen," and a sanity and homely simplification give his book a genuineness so often lacking in the preten-

tious pedantry now published to prop up promotions. Some may regret an absence of unifying themes and the subdivision of chapters into little segments, but to coordinate many different scholars' conclusions about the common law with his own independent interpretations of the major documents of constitutional law is not an easy thing to do, and he has done it well. Like Keeton, Hogue, too, let go his feelings toward the twentieth century. The Briton denounced the past hundred years for having "legislated out of English law" "Christian usages and Christian doctrine"; while the American condemned "modern ideas of sovereignty which admit no limitation on the power of ruling." Instead of a "self-directing bureaucracy" of experts and agencies, Hogue rightly urges a return to the rule of law, and he pits "the doctrine of the supremacy of law" against "a doctrine of government regulation by administrative orders." Ironically, the writs issued out of that "writ-shop," the medieval chancery, as both Hogue and Keeton explain, were just such administrative orders. In due season they, too, became part of common-law procedure.

Yale University

WILLIAM HUSE DUNHAM, JR.

SEEKRIEG UND SEEPOLITIK ZWISCHEN ISLAM UND ABENLAND:
DAS MITTELMEER UNTER BYZANTINISCHER UND ARABISCHER
HEGEMONIE (650-1040). By *Ekkehard Eickhoff*. (Berlin: Walter de
Gruyter & Co. 1966. Pp. xi, 438. DM 72.)

SOME fifteen years ago I published a study of naval power in the Mediterranean during the early Middle Ages. It is thus with particular pleasure that I call to the attention of the scholarly world this volume by Ekkehard Eickhoff, which promises to be the definitive work on this subject for a long time to come. Showing a rare grasp of all primary Byzantine, Arabic, and Western sources and a mastery of almost all relevant secondary materials, the author has told in full detail the story of the naval wars between the Islamic world, on the one hand, and Byzantium and the West, on the other, between A.D. 650 and 1040. The story he unfolds is essentially one of an Arab assault between A.D. 650 and 750, of Byzantine resurgence in the next century, of a ninth-century western Muslim conquest of Crete and Sicily despite Byzantine resistance, of the counteroffensive of Byzantium that gained mastery in the eastern Mediterranean about 950, and of the rise of Italian naval power that won the western Mediterranean from Islam in the eleventh century prior to the crusades. Particularly impressive are his chapters on Muslim and Byzantine ship types, naval tactics, and naval organization. I also admired his analysis of the material and religious bases of western Muslim naval strength in the Maghrib in the ninth and tenth centuries, which explains much of its success in the Mediterranean.

Though the naval and political aspects of his narrative seem impeccable, Eickhoff's grasp of economics seems less sure. He fails to give sufficient emphasis to the effect of Byzantine trade controls on Western Europe's economic life, especially between the reigns of Justinian II and Leo V, following the lead of historians of Byzantium who have preferred to ignore this subject. And he accepts too unquestioningly Lombard's thesis concerning the importance of Muslim

gold to Western Europe between the seventh and the eleventh century, unaware of how untenable this thesis is. The fact is that to the tenth century those Islamic regions in closest contact with Northern and Western Europe—the Maghrib, Spain, and Sicily in the West, and Iraq, Persia, and Turkistan in the East—were on a silver not a gold standard. Only in the tenth century did trans-Saharan gold reaching the Maghrib and Spain and African gold reaching the Indian Ocean change this state of affairs. Even then it was not until the eleventh century that Muslim gold reached Western and Northern Europe in appreciable amounts, a fact confirmed by coin hoard finds which reveal only about a dozen gold dinars out of a total of approximately 100,000 silver dirhems that have been unearthed.

Economics aside, Eickhoff is to be congratulated on his valuable volume. Let us hope it banishes once and for all from American textbooks the Pirenne-inspired error that the Muslims controlled the Mediterranean from the mid-seventh to the mid-eleventh century. And let us further hope that it forces historians of Byzantium to pay increased attention to those naval and maritime aspects of Byzantine history that they have tended to minimize.

University of Texas

ARCHIBALD R. LEWIS

FROM PACHOMIUS TO IGNATIUS: A STUDY IN THE CONSTITUTIONAL HISTORY OF THE RELIGIOUS ORDERS. By *David Knowles*. [The Sarum Lectures, 1964–5]. (New York: Oxford University Press. 1966. Pp. 98. \$2.40.)

SCHOLARS and students will both welcome this small book by the foremost living authority on monastic history. It is concerned with the organization and government of the religious orders and concentrates on the period between 994, when Odilo became abbot of Cluny, and the death of St. Dominic in 1221. After an introductory chapter entitled “From Pachomius to Benedict of Aniane,” it includes chapters on Cluny, the new orders of the eleventh century, the Cistercians, monasticism in the twelfth century, the Friars Minor, and the Order of Preachers. The chapter on the “Transition to the Modern World” deals mainly with Santa Giustina of Padua and the Jesuits, whose organization summed up in many respects the earlier developments. There is a final chapter on the doctrine of obedience, which underlay the entire structure of monastic government.

Distinctive features of the organization of each order are clearly delineated. Thus, for Cluny, Professor Knowles emphasizes its dependence on the Holy See and direct control over all members of the order; for the Cistercians, their regular visitations, annual general chapters, and system of lay brothers; for the military orders, their use of the elective principle and regional organization; and so on. He discerns an evolution, almost a dialectic, by which the fully developed religious orders emerged. The friars thus remained in this respect, he says, “within the direct line of development from the monastery.”

This is a pioneering study, covering much of disputed scholarly ground, and some scholars may not agree with every point of the analysis and presentation. Some of the similarities, differences, and influences between the various orders may, in particular, be revised by future research. The book is, also, a masterpiece

of compression, and there are inevitably some simplifications and omissions. The feudal character of Cluny is accepted without dispute, for instance, and the complete omission of St. Robert of Molesme may distress some historians of the origins of Cîteaux. The book is fully abreast of recent scholarship, however, and will provide a clear and sure starting point for further research in this comparatively neglected area of monastic history.

Harvard University

GILES CONSTABLE

DUMBARTON OAKS PAPERS. Number 19. (Washington, D. C.: Dumbarton Oaks Center for Byzantine Studies, Trustees for Harvard University; distrib. by J. J. Augustin, Publisher, Locust Valley, N. Y. 1965. Pp. xi, 265. \$12.00.)

THE main part of this book is devoted to "Byzantium and the Mission to the Slavs," which was the subject of the 1964 symposium at Dumbarton Oaks. The symposium commemorated the eleven hundredth anniversary of the mission of Saints Cyril and Methodius to the Moravians.

George Ostrogorsky's paper, "Byzantine Background of the Moravian Mission," emphasizes the importance of the Slavic immigration in the Balkans and connects it with the establishment of the themes that, according to the author, restored Byzantine authority in the territories occupied by Slavs. In the wake of this restoration came an expansion of Byzantine influence in Southeastern Europe; the mission to the Moravian Slavs is an aspect of this expansion. Cyril and Methodius were well acquainted with the Slavic language because Thessalonike, their native city, was bilingual, and the two peoples, the author points out, coexisted harmoniously in that city and its vicinity. George C. Soulis contributed "The Legacy of Cyril and Methodius to the Southern Slavs." The paper discusses the influence of the disciples of the two saints, Clement, Nahum, and others after they were forced out of Moravia. Their arrival in Bulgaria came at a crucial moment and had far-reaching effects on the religious and cultural development of Bulgaria, Serbia, Rumania, and finally Russia. "The Heritage of Cyril and Methodius in Russia," by Dimitri Obolensky, describes the potent influence of Byzantine Christianity on the Russian mind in later centuries. More detailed and specialized is Antonin Dostal's "The Origins of the Slavonic Liturgy." Among other things that are too technical to be mentioned here, the author sets out to prove that the Macedonian Slavic dialect was the first medium for the development of Slavonic ecclesiastical literature. The beginnings were based upon a program of translations envisaged by Cyril himself. In its actual implementation, the program sought to approach the Slavic mind, as it was in the ninth century, and the Christian texts are often deliberate adaptations rather than accurate translations. Their influence is recognized to be great. The author thinks, however, that it is probable that a Western liturgy had been introduced into Moravia before the advent of the two brothers from Thessalonike.

This volume also includes five other contributions that are either peripheral or unrelated to the theme of Byzantium and the Slavs. The notes at the end of the volume deal with excavations in Istanbul, radiocarbon dating of wooden beams from Saint Sophia, Nicetas David Paphlago, and Constantine Acropolites.

University of Texas

G. G. ARNAKIS

TRAVAUX ET MÉMOIRES. Number 1. [Centre de recherche d'histoire et civilisation byzantines.] (Paris: Éditions E. de Boccard. 1965. Pp. 461. 100 fr.)

THE *Centre de recherche d'histoire et civilisation byzantines* has now undertaken this more or less annual venture. Its director, the eminent Paul Lemerle, insists in his preface that "Il s'agit donc pas d'un nouvelle Revue, mais d'un Recueil," since it will not include any reviews or bibliographies. Its next number will, however, offer a résumé of recent Soviet Byzantinology. Regardless of how it is designated or categorized, this new venture can only be welcomed, especially in view of the promising standards set by the first volume.

Nearly the first half of it is devoted to a book-length monograph by Hélène (Glykatzki-)Ahrweiler, "L'histoire et la géographie de la région de Smyrne entre les deux occupations turques (1081-1317) particulièrement au XIII^e siècle." This exhaustive memoir is designed to fill in gaps in general works, and, at the same time, it is a very important study of regional problems within the context of a crucial era in the history of the Byzantine Empire. Perhaps the most useful aspect of the study is its section on civil and military administration, providing an illuminating case study of a pivotal Asiatic Theme during the period of the disintegration of the Theme System. This is a contribution of the first magnitude to the literature on Byzantine government and history during the post-Macedonian epoch.

In the second part, "Dossiers," Ivan Dujčev examines "La chronique byzantine de l'an 811." This important account of the defeat and death of the Emperor Nicephorus I at the hands of the Bulgars was edited and published by Dujčev some twenty years ago. It has been the subject of considerable philological and historical debate, and, in view of that discussion, Dujčev has prepared a new critical text, with full apparatus, a French translation, and a commentary. In this same section Lemerle contributes a searching analysis of the rebellion of Thomas the Slav in the early ninth century, and of the source accounts for it. While avoiding any sweeping interpretations or conclusions, he provides a concise and indispensable guide to the thorny problems involved in this obscure but extremely important episode in Byzantine history.

The final section consists of six "Études et documents": Jean Gouillard discusses the necessity for coherent consideration of heresy in Byzantine history as one broad phenomenon, sketching what he regards as the patterns that make up its totality to the twelfth century; Nicolas Svoronos argues for the attribution to Manuel I (1143-1180) of an obscurely dated chrysobull and then uses it as a point of departure for an important analysis of that emperor's policies regarding ecclesiastical properties; Charles Astruc surveys the manuscript tradition of the rhetorical writings of the Nicaean Emperor Theodore II Lascaris (1254-1258), preparatory to his critical edition of them; David Jacoby discusses an aspect of Venetian administration during the fourteenth and fifteenth centuries in the republic's territories around Coron and Modon, the *zovaticum*, and its relationship to Byzantine institutions; Jean Verpeaux writes on the evolution of ranks and hierarchies under the Palaeologan dynasty; and Irène Beldiceanu-Steinherr evaluates the Turkish sources for the reign of Sultan Murad I (1362-1389) and argues that the initial Turkish conquest of Adrianople, in 1369, was the work of independent

frontier commanders, the city and its region not coming under Murad's direct control until 1376-1377.

The consistently high quality of these studies suggests that this new publication will, in the years to come, serve as a very important forum for contributions to the field of Byzantine studies.

University of Wisconsin

JOHN W. BARKER

THE SHAPE OF MEDIEVAL HISTORY: STUDIES IN MODES OF PERCEPTION. By *William J. Brandt*. (New Haven, Conn.: Yale University Press. 1966. Pp. xix, 177. \$5.75.)

HISTORIANS, more than most people, are aware of the crucial importance of unconscious presuppositions in determining both what is perceived and how this perceptual material is organized. Professor Brandt, in a thoughtful and interesting book, has attempted to delineate the modes of perception that determined the way medieval men saw and understood both natural phenomena and human actions. His study is introduced by an analysis of the ways of perceiving the natural world implicit in the writings of Isidore, William of Conches, and Albert the Great.

The main portion of the book is devoted to a study of English chronicles of the twelfth through the fourteenth century. From these sources the author concludes that there were two concurrent and independent modes of perception. The first of these, the clerical, is characterized by a nontemporal attitude toward its material. Each entry is considered as a discrete unit and exhibits a triadical structure: a pre-existing situation, disturbed by some outside agent, is followed by a resolution, which may or may not constitute the ground of a new action. In these clerical chronicles there are no narrative intent, no continuity, no causal sequence of action.

The second type of perceptual framework, the aristocratic, is, on the other hand, purely narrative. The interest is in the story itself, which celebrates heroic types engaged in appropriate action. Brandt argues well that the aristocratic chronicles exhibit not only the chivalric value system, but a perceptual mode determining which things were noticed at all, and how they were ordered, understood, and evaluated.

This book has many faults: its title does not correspond to its contents; its parts are not coherent; it fails to discriminate between modes of perception and modes of presentation; it is arbitrary in imposing the author's analysis of structure on the material being considered; it does not take adequate account of the similarity between the medieval works it discusses and classical works of the same sort (and hence it assumes what it ought to prove: that there was such a thing as the medieval mind); and its discussion of medieval physics is based on a dangerously limited number of sources. Still it is often illuminating and frequently provocative. The book is clearly the result of a fine mind wrestling honestly and imaginatively with important problems. Its greatest value is as a starting point for further discussion and clarification rather than as a definitive solution of the questions it treats.

University of Southern California

RICHARD C. DALES

FINANCES ET COMPTABILITÉ URBAINES DU XIII^e AU XVI^e SIÈCLE: COLLOQUE INTERNATIONAL, BLANKENBERGE, 6-9-IX-1962. Volume I, ACTES; Volume II, ACTES—PLANCHES. [Collection Histoire, Number 7.] ([Brussels:] Pro Civitate. 1964. Pp. xx, 429; unnumbered.)

IN 1961 the *Crédit Communal de Belgique*, for a century a model organism for local government finance, established the *Centre Pro Civitate*, itself an excellent model for bringing local history to its highest level of significance. An exacting standard was set, for example, by its striking volumes of photographs of the scale model towns built for Louis XIV's military. This book, too, exceeds all reasonable expectations: municipal account keeping seems a narrow subject, and collections of symposium papers are seldom promising, yet the book, besides being pleasant, informative, and stimulating, is one of broad scope and many-sided relevance.

Between the *exposé général* by Marinette Bruwier and an unusually thought-provoking summation by J. Dhondt are thirteen papers (too many to detail here), followed by discussion. Volume II is a packet of thirty-six prints of documents. The main shortcoming—geographical imbalance—partly redressed by three appended *communications*, is more than offset by variety of approach (both wide and surprisingly complementary) and an admirable breadth of view. With varying emphasis, institutions, taxation, political context, and much else are judiciously considered for various locales. Sources awaiting study are usefully described, but not redundantly. The question of parchment versus paper is attacked from several angles, and its ramifications explored; the narrow matter of dating the change is here in the hands of historians, not antiquarians. Local responsibility for fortifications, creating a need for new systems of taxation, credit, and accounting, has its evidence neatly fixed. When a case seems to be building for municipal independence, princely control through powers of audit is deftly established for the important examples of Flanders and Brabant by Frans-Laurent Blockmans (who died only two months later, appropriately eulogized here by H. Van Werveke).

Polish scholars—A. Gieysztor on Poland and H. Samsonowicz on the Hanse towns—occasion a lively discussion of the North's failure to adopt double-entry bookkeeping, revolving around Roberto Lopez' theory of cultural inertia (so long as old methods remain adequate there is insufficient motive to adopt new ones). It is a measure of the quality of this volume that the discussions are as readable and informative as the papers. In one amusing exchange Dhondt tells the director of the *Crédit Communal* (who has suggested a line of research) that "les historiens sont des gens bizarres, ils n'étudient que ce qu'ils étudient." These particular historians have studied a narrow subject and made it broad and meaningful in a pleasant and profound volume that every historian of Europe should read.

Almuñécar, Spain

CHARLES H. CARTER

THE FRENCH APANAGES AND THE CAPETIAN MONARCHY, 1224-1328. By Charles T. Wood. [Harvard Historical Monographs, Number 59.] (Cambridge, Mass.: Harvard University Press. 1966. Pp. viii, 164. \$5.50.)

THIS work argues that the great landed endowments created by the later Capetian kings for their numerous sons were a distinctively monarchical phenomenon. By

no means solely "feudal," much less a capitulation to disintegrative impulses, the appanages originated in accord with a developing recognition of the nonproprietary nature of the realm. The "reversion clause" of the charters proves royal solicitude for territorial integrity; the appanagists constituted an exalted class of king's vassals whose interests rarely strayed far from the throne, which came under their close influence in the troubled last years of the direct Capetian line; while in matters of jurisdiction and administration the princes tended to policies that the author characterizes as more royalist than the king's.

Within these constitutional limits the discussion is successful and sometimes illuminating (for instance, on the exclusionist politics after 1314 and on the princes' attitudes toward their estates). Wood's careful analysis of the appanage charters, which he has studied much more thoroughly than previous scholars, makes an especially valuable contribution. Less persuasive is his insistence, against Robert Fawtier, that the appanages were no more feudal than the "forms" in which they were "clothed." Wood has not investigated the relation between feudal (non-royal) and royal reversion; nor does he sufficiently allow for feudal evolution within monarchy and appanages alike, preferring to regard feudalism as a stable and separate "force." Even considering the limited theme of the book, the incomparable administrative records of Alfonse of Poitiers seem inadequately exploited. They reveal a more pragmatic policy than Wood appears to admit. A more rounded interpretation of the early appanages would have set the later royalist tendencies in a better perspective. Readers may find it helpful to know that it is the familiar translation of Joinville, not the original text (as implied), that is used; also that some of the important documents cited from manuscript have been published (for example, by Artonne, Boutaric, and Langlois). Wood has evidently tried hard to produce an uncluttered, readable book. The result is a good addition to the literature on Capetian kingship.

Swarthmore College

THOMAS N. BISSON

BATTLE ROYAL: A NEW ACCOUNT OF SIMON DE MONTFORT'S STRUGGLE AGAINST KING HENRY III. By *Tufton Beamish*. With a foreword by *Sir Charles Petrie*. (New York: Barnes and Noble. 1966. Pp. 285. \$6.00.)

IN evaluating Colonel Sir Tufton Beamish's excursion into thirteenth-century history, one is tempted to exclaim "Jolly good try, old chap!" and let it go at that. The reader would suppose from the subtitle that here is a new approach to the turbulent reign of the incompetent Henry. *Battle Royal* is, of course, a new account, but it adds little to the existing knowledge of the struggle between the King and the baronial opposition led by his strong-willed brother-in-law, Earl Simon. Beamish has put together a series of stereotyped and not very convincing episodes which are, to put it bluntly, the work of a "talented amateur." While, as Sir Charles Petrie has stated in his generous foreword, the book is intended "primarily for the layman," the intended audience should be wary of judging thirteenth-century England by what is found in these pages. The author has been by turns a soldier and a politician, but unfortunately, neither of these professions seems to have pre-

pared him to deal in realistic terms with events occurring some centuries before he became the honorable member for Lewes. The reader is assured, for example, that "at the Norman Conquest England was just a barbaric offshore island," and that in 1257 "The Holy Roman Empire was . . . almost a myth."

But let us take the area in which Beamish, because of his extensive military experience, might be expected to provide some new insights. Regretfully such is not the case, and we are solemnly informed that "at this period the foot soldier was of little importance" and "rarely made an effective fighting force"; that the provision of a reserve by Earl Simon at Lewes was "a most unusual but sensible step"; that armies engaged with "the almost comic ritual of a medieval battle"; and that "the idea of adding science and imagination to brute force was new." It is hard to escape the conclusion that the author simply has not kept abreast of current scholarship in the field of military historiography and that *Battle Royal* is of a genre more closely related to the 1920's than to the 1960's.

University of North Carolina, Greensboro

JOHN BEELER

KÖNIGTUM UND LANDFRIEDE IM DEUTSCHEN SPÄTMITTELALTER. By *Heinz Angermeier*. (Munich: C. H. Beck'sche Verlagsbuchhandlung. 1966. Pp. xvi, 592. Cloth DM 63, paper DM 58.)

THE function of the medieval monarch as the guardian of *pax* and *justitia* lent lofty sanction to the royal authority and could notably enhance its executive powers. But justice also required the preservation of the legal rights of the subject and of the estates of the realm. Hence the enforcement of public law and order depended in part upon the degree of cooperation between the monarchy and the estates. By the middle of the thirteenth century the territorial principalities of Germany were far advanced toward *de facto* autonomy at the expense of the imperial monarchy. It is at this point that the author of this thoughtful and weighty study begins his analysis of the various attempts of the German emperors to legislate and enforce public order in the late medieval period. The successful application of a *pax regia* throughout Germany would have provided the rulers with a powerful entering wedge into the territories and jurisdictions of the princes of the Empire. But the issue was so momentous that neither the monarchy nor the estate of princes was prepared to yield. In the protracted conflict that ensued over the control and administration of the *pax publica* the monarchy was defeated, and law and order experienced a grievous decline.

The author contends that the imperial monarchy did not lose this crucial contest through the weakness or incapacity of the individual rulers, but by reason of their tendency to concentrate primarily on the consolidation and expansion of their dynastic possessions and by their inability to contrive institutional ways and means of exerting influence on the problem of public order in Germany as a whole. The reliance of the monarchy on local courts of law and on local assemblies of estates for the administration of the *Landfriede* also tended to emphasize the role of the territorial princes as guardians of public order at the expense of the king. Nor was the Diet, convulsed by disputes between the estates and seriously lacking in executive vigor, a convenient organ for the elaboration and enforcement of legisla-

tion designed to ensure the *tranquillitas publica*. Elsewhere in Western Europe the *jurisdictio generalis* of the ruler had been institutionalized and employed as a weapon against the overmighty subject. But the German *Landfriede* was based on the recognition of the existing rights and privileges of the various participants. Hence the right of indulging in private feuds was paradoxically acknowledged in the successive *Landfrieden* until the fifteenth century. Thus the reciprocal and conditional character of the *Landfriede* diminished its value as a pillar of royal authority in Germany. This consideration reduces the force of the author's argument that the enforcement of a *pax regia* in Germany constituted a viable alternative to the Habsburg policy of expanding the patrimonial territories of the dynasty.

The author writes with great knowledge and persuasiveness, and only occasionally does one pause to seek further information or to question a conclusion. I would have welcomed a critical analysis of the local operation of the *Landfriede*, and the influence of Roman law on the imperial concept of the *Landfriede* in the fifteenth century might well have been elucidated. Finally, the author's criticism of the opposition of the Habsburg dynasty to projects of legal and constitutional reform should perhaps have been tempered by the reflection that the reform proposals of the princes almost invariably involved a further invasion of the dwindling area of imperial jurisdiction. This stimulating and scholarly study of a vital historical theme, however, offers the interested reader an opportunity to institute intriguing comparisons with the techniques applied by other European monarchies in the suppression of public disorder during the late medieval period.

McGill University

C. C. BAYLEY

PADUA IN THE AGE OF DANTE. By J. K. Hyde. (New York: Barnes and Noble. 1966. Pp. xi, 349. \$8.75.)

THE republican period in Paduan history is well defined and is notable for an intellectual output equaled by no other Italian city of the time except Florence. Since the history of Italy and of the Renaissance has been written so largely in Florentine terms, this solid, penetrating, and judicious study of Paduan culture is most welcome. From the overthrow of Ezzelino da Romano in 1256 to the installation of Marsiglio da Carrara as *signore* in 1328, Padua was governed by political leaders dependent on the votes of a Great Council which represented a relatively wide distribution of political rights. The rulers "had to respect the ideal of government in the common interest, *ad communem* and not *per partem*." In these same decades there appeared in Padua the first of the Italians to take the attitude toward ancient literature that Petrarch preached so successfully a generation or so later. These pioneer humanists were all members of the legal profession, which formed a sort of ruling class in Padua.

The core of Professor Hyde's study is his analysis of the composition of this class and its relation with other classes. He describes the city's economy, showing that it had no important industries producing for export and no substantial body of merchants engaged in foreign trade. It was the center of moneylending, marketing, manufacturing, and government for a region of fertile farms and vineyards

owned by Paduan citizens. The structure of the constitution placed power in the hands of the College of Judges and the Guild of Notaries, without question the greatest of the guilds both in numbers and influence. Hyde makes clear that the economic base of the judicial class was the ownership of land cultivated by tenants. Notaries of modest wealth engaged in various trades and enrolled in other guilds. Partly because of their wealth and numbers, partly because their profession placed them at the center of civic life, judges and notaries dominated the politics of republican Padua, although they had to share power with landowners who were considered nobles and whose families had not been entirely absorbed into the communes.

Hyde buttresses this analysis of the ruling classes by tracing the careers and family connections of individuals who appeared publicly important. He has culled material for that purpose from notarial records and other archival sources as well as from the chronicles. Because he is disinclined to draw the conclusions that reveal the importance of his evidence until after he has presented it, his presentation is sometimes heavy going.

By diligent use of detail and by appreciation of the general movements of the time, Hyde succeeds brilliantly in disclosing the links between the social and political position of the ruling class on the one hand and the range and limitations of its contributions to literature and learning on the other. He suggests that the failure of Paduan republicanism to survive longer was partially owing to those cultural limitations. In spite of the revival of Aristotelianism by Pietro d'Abano and Marsiglio Mainardini and in spite of the germinal labors of Albertino Mussato and others in imitating classical literary forms, a patriotic republican devotion to the city-state was not strengthened by these learned admirers of antiquity. Instead Hyde is impressed by the growing emphasis in the learned writers on class consciousness and by the increasing bitterness between old families and new. Devotion to personal and family status and the strain of the war with Verona enabled the Carrara to end the period during which republicanism had been a vital force in Padua.

Johns Hopkins University

FREDERIC C. LANE

THE ORGANIZATION OF WAR UNDER EDWARD III, 1338-62. By H. J. Hewitt. ([Manchester:] Manchester University Press; New York: Barnes and Noble. 1966. Pp. ix, 206. \$7.50.)

Mr. Hewitt's new study is in some respects not a book at all but a series of loosely connected essays on a variety of subjects. Despite the fact that all the topics discussed deal with the impact of the Hundred Years' War on French and English society, they do not fit together. It would be possible also to quarrel with the author's style, for this is not the smooth narrative of his splendid *Black Prince's Expedition*. Yet I would hazard the opinion that nothing more significant in the field of English medieval military studies has appeared in the last half century. It is an exciting book; indeed, a challenging one. Almost every page shows what can be done, and what remains to be done, to show the effects of war on the people of France and England. Here is no ill-mannered quibbling over this

or that aspect of institutional development, no "reconstruction" from inadequate sources of half-forgotten battles. Here is war described in a modern sense as it affected the lives not only of the men who fought in it but also of those who suffered its ravages and of those who stayed at home and provided the logistical support for armies operating beyond the narrow seas.

A review, unfortunately, does not provide sufficient space to indicate more than the fields that the author feels must be studied more fully for an adequate understanding of medieval warfare. More than a decade ago I noted that war was second only to agriculture as a factor in the lives of medieval Europeans, but, not surprisingly, few attempts have been made to relate warfare to the activities of people other than the actual combatants. But the effects of collecting and forwarding supplies, of recruiting and moving large numbers of troops, of requisitioning ships for overseas transportation, of providing for home defense, of official efforts to influence public opinion can be profitably investigated to give a broad picture of "the practice of war," as distinguished from "the art of war," in the author's words.

It must be admitted that the materials for analyzing such aspects of the organization of war are far more plentiful for the mid-fourteenth century than they are for earlier periods. Indeed, it might be argued that prior to the reign of Edward III the sources simply do not exist for inquiries in such detail. But it also might be fairly asked if all of us do not too frequently let our attentions be diverted to problems that are merely academic. All students of the medieval English scene, particularly those interested in "the practice of war," are indebted to Hewitt for this thoughtful, perceptive, fascinating, and extremely significant study.

University of North Carolina, Greensboro

JOHN BEELER

STUDIES IN MEDIEVAL AND RENAISSANCE HISTORY. Volume III.

Edited by *William M. Bowsky*. (Lincoln: University of Nebraska Press. 1966. Pp. 313. \$7.95.)

THIS is the third volume of a newly founded annual designed principally to publish articles close to monograph length. The present issue continues the practice with fields divided between things medieval and Renaissance. The earlier era is represented by Andre Joris' study of northern Gaul during the Merovingian period, Richard Sullivan's assessment of the effect of Christianity on Bulgaria at the time of the conversion of Khan Boris, and Craig Fisher's useful analysis of the beginnings of historical writing among the Pisan clergy in the eleventh and twelfth centuries. For the Renaissance we have Mrs. de Roover's careful description of the business career of that eminent *quattrocento* Florentine silk entrepreneur, Andrea Banchi, and D. Chambers' sympathetic portrayal of what Jakob Burckhardt aptly referred to as "the secret misery of the prelates": indebtedness.

There is material of great value in the three medieval studies, but Joris' article must stand as a bleak catalogue of facts in which he is never tempted to effect telling correlations. Sullivan's work contains valuable delineations of questions troubling the mind of those newly converted to Christianity, but is not overly sensitive

to the role of ritual in these early medieval cultures. His arresting arguments are blunted after almost ninety pages. Had Fisher, in his historiographical inquiry, availed himself of the studies of N. Rubinstein, G. Arnaldi, and others, he would have transformed a most useful treatment into a revealing amplification of that shaded continent between communal psychology and nascent historicism. The two Renaissance studies are aptly done and contain few surprises: De Roover's findings fit nicely on the ideal parabola of Florentine *quattrocento* economic conditions, while Chambers is urbane in his treatment of the problems members of the *Curia* had keeping up with the cardinals. Surely there is need for such a journal, although it would seem this one suffers from two rather common defects: first, that, to be published, a study must present either new facts or a novel interpretation; second, that "solid history" must resemble an "A" examination, wherein legions of "real" facts are marshaled in response to "unreal" questions. A journal of this kind has an unrivaled opportunity to encourage syntheses, review findings in related fields, make discoveries in the arts and music available to the researcher occupied with medieval and Renaissance political behavior, and so forth. Some boldness might be in order, lest Nietzsche's observation be proven true: One studies history to learn how to accept boredom.

University of Rochester

MARVIN B. BECKER

RENAISSANCE HUMANISM, 1300-1550. By *Frederick B. Artz*. ([Kent, Ohio:] Kent State University Press. 1966. Pp. ix, 103. \$5.00.)

THIS book will provide students with a brief but comprehensive introduction to the whole course of the humanist movement from its origins in the Age of Petrarch to its culmination in that of Erasmus. It is a topographical survey in which only the peaks are noted, but the main outlines emerge all the more clearly.

Professor Artz rightly emphasizes the enormous critical labor involved in the textual rehabilitation of all that survived of ancient Latin and Greek literature, but he also makes it clear that it was the content as well as the form of the classics that aroused the enthusiasm of the humanists, and that their own contribution was not limited to philology. In Italy antique moral philosophy enabled the humanists to create a lay morality for a secular urban society, while in the North the Christian humanists found in the combined study of pagan and Christian antiquity the inspiration for religious reform. On both sides of the Alps the humanists revolutionized the ideals and methods of education. Above all they performed the invaluable service of bringing back into the mainstream of Western civilization its antique heritage. "This was a heritage," Artz concludes, "so valuable in itself that human life would be poorer without it, and also a heritage so fraught with power to educate and stimulate that the permanent loss of it would have been the annulment of an inestimable agency in the development of the human faculty." It is heartening to have this truth, no longer universally accepted as self-evident, thus reasserted.

University of Western Ontario

WALLACE K. FERGUSON

THE UNIVERSAL CHRONICLE OF RANULF HIGDEN. By John Taylor.
(New York: Oxford University Press. 1966. Pp. x, 198. \$6.10.)

JOHN Taylor's attractive study of Ranulf Higden's *Polychronicon* involves a wide range of historical literature reaching backward from the fourteenth century, when Higden was writing, and forward into the sixteenth century when his chronicle, with continuations, was still popular in England. Taylor has marshaled his materials with skill and has brought to bear an expert knowledge of medieval historical writing that enables him to get at the sources behind the sources. Consequently, without wandering from his subject, Taylor has written a lively introduction to an important medieval chronicle. While placing Higden's work in its relationship to other sources, Taylor appraises the emphases and deficiencies of Higden's vast compilation which fills nine volumes in the Rolls Series edition (1865-86).

Higden lived for about sixty years in the Benedictine abbey of St. Werburgh, Chester. Remote from Westminster, which he visited but once, he had little to say about public affairs in his own time, and he contributed little to a knowledge of England in the thirteenth century. The popularity of his writing rested on his power of narrative when dealing with classical antiquity, his power of description when dealing with geography, especially that of the British Isles, his succinct accounts of the early bishoprics of England, and the encyclopedic sweep of his interests. Taylor labels him "a 'classicizing' monk, something of an antiquary." The large number of surviving manuscript copies of Higden's chronicle sufficiently proves his popularity. Other chroniclers, moreover, wrote continuations to bring Higden's chronicle up to date; these are particularly valuable for the reign of Richard II. John Trevisa, a contemporary of Wyclif at Oxford, completed an English translation of Higden's *Polychronicon* in 1387, and in 1482 William Caxton printed the translation, to which he added a continuation of his own making. Taylor explains the exceptional popularity of the *Polychronicon* by noting how well Higden reflected the changing outlook of the educated clergy of fourteenth-century England who were increasingly interested in the ancient world.

Taylor has listed every known version and manuscript copy of his subject and in other ways has supplied the scholarly apparatus that will enable scholars to use Higden's chronicle with confidence. Taylor's book is a model of what can be done to provide the historical profession with a critical and interesting appraisal of an important historical work.

Indiana University

ARTHUR R. HOGUE

VIZCAYA EN EL SIGLO XV: ASPECTOS ECONÓMICOS Y SOCIALES. By José Angel García de Cortázar y Ruiz de Aguirre. (Bilbao: Ediciones de la Caja de Ahorros Vizcaína. 1966. Pp. 479.)

BECAUSE regional studies in depth offer a principal hope of carrying medieval and early modern Spanish history beyond timeworn and often dubious generalizing, and because historians have neglected the admittedly important Basque provinces, this detailed reconstruction of economic growth and social change in fifteenth-century Vizcaya is of exceptional value.

Cortázar draws upon unused material at Simancas, Valladolid, and the Vizcayan archives to show how, after 1400 and notably under the Catholic kings, the hitherto backward, isolated, feud-torn, heavily rural *Señorío* developed into one of the most economically advanced parts of Castile—a center of shipbuilding and seafaring, a major entrepôt for wool, iron, iron products, textiles, and other commodities, and Spain's chief base for its vital Northern European trade. After surveying the province's geographical and political subdivisions, and the rural-urban distribution of its 67,000 inhabitants (by about 1500), the book identifies the decisive factors in the great transformation: the suppression by Ferdinand and Isabella of rural disorder; the permanent wheat shortage, only partially offset by stock raising and fishing; the abundance of iron and manufacturing skills; the funneling through the *Señorío*, by the merchants of Burgos, of the Mesta's Castilian wool clip destined for Flanders and England. Vizcayan life, in short, came to depend for survival upon commercial exchange, and, although statistics are scarce, Cortázar demonstrates this through extensive examination of the far-flung connections with Castile, the Mediterranean, and, above all, the Atlantic North—Flanders, England, Normandy, Brittany, and Gascony. The culmination came in 1511 with the establishment, after bitter struggles with Burgos, of the Bilbao *Consulado*. A concluding section on Vizcayan society minimizes Jewish mercantile presence (another element in the Burgos-Bilbao conflict?), analyzes the complicated royal revenues, and traces the process by which the old feuding noble families were drawn into commercial and industrial activities to become rivals of the great merchants and shipbuilders of Bilbao and the Nervión estuary for control of municipal government. Over a hundred pages of unpublished documents, five maps, and a useful bibliography are provided; the last might well have included the names of Lapeyre, Chaunu, and Van Klaveren.

While no startling revisions emerge, the book does clarify as never before the main lines of Basque socioeconomic evolution at the end of the Middle Ages, and ought now to be extended into the changing Spain of the Habsburg era. Finally, it can be commended as a model for similar much-needed provincial studies of other parts of the peninsula.

University of Virginia

C. J. BISHKO

DIE VERWALTUNG DES ORDENSSTAATES PREUSSEN: VORNEHM-
LICH IM 15. JAHRHUNDERT. By *Peter Gerrit Thielen*. [Ostmitteleuropa
in Vergangenheit und Gegenwart, Number 11.] (Köln Graz: Böhlau Ver-
lag. 1965. Pp. viii, 196.)

THIS excellent monograph deals with the Teutonic Order's administrative methods, measures, and practices during the period 1410–1449. An analysis in depth compensates for the study's narrow subject matter and the time span covered. Thielen discusses in detail not only the administrative operations of the territory's central authority, but pursues them beyond the provincial and district *Komtureien* down to the local order castle and manor level (*Ordenshaus* and *Ordenshof*). His study achieves historical perspective by references to comparable earlier conditions

and by a brief summary of the administrations after 1449 to the secularization in 1525.

As editor of *Das Grosse Zinsbuch des Deutschen Ritterordens (1414-1438)* (1958), which reveals the sources and types of income and services as well as the operational procedures up and down the chain of the order's economic command, Thielen was led to uncover new primary materials and to re-evaluate previously known ones. In the first chapter of the current book he presents a critical survey of the available archival sources relevant to the period, most of which are on deposit in Göttingen. While he suggests that additional documents may be located in Polish archives, he does not indicate their possible volume and importance. A twelve-page bibliography restricted to printed works dealing with various phases of the order's administrative problems underlines the need for the kind of synthesis that the study offers.

During the period following the Teutonic Order's defeat at Tannenberg, inflicted by the Poles in 1410, three significant changes took place. After centuries of expansion the order was put on the defensive; this, in turn, led to a thorough administrative reorganization to cope with the altered circumstances; and it gave the hitherto politically impotent towns of the territory the opportunity to contend for a share in the affairs of the state. The realization that Prussia was the only monolithic authoritarian state in Europe approximately two centuries after estates (under whatever name) had been formed with more or less active and lasting effect everywhere else, has a sobering effect on anyone familiar with Brandenburg-Prussia's historical development from the time of the Great Elector to the recent past.

In a fifty-five-page appendix are listed all the order's officials of the period whom Thielen was able to identify by name. It enabled him to establish various relationships, rates of mobility, and promotions. A model of its kind, the list will prove indispensable to anyone working in the field. For so thorough a study it is regrettable that the author failed to offer any comparable annual budgets of the order, sample accounts from some *Komtureien*, trade balances, or the amount of tribute paid to Poland. Also, one misses a map.

Polytechnic Institute of Brooklyn

FELIX F. STRAUSS

JOHN HUS AT THE COUNCIL OF CONSTANCE. Translated from the Latin and the Czech with notes and introduction by *Matthew Spinka*. [Records of Civilization: Sources and Studies, Number 73.] (New York: Columbia University Press. 1965. Pp. xii, 327. \$8.75.)

THE conciliar movement of the later Middle Ages has, in the past few years, been the object of much serious study in Western historiography. Dramatic incidents of the whole movement were the cases of John Huss and Jerome of Prague, both of whom were tried and condemned at Constance as heretics. Huss's trial and execution were the more significant in that they were the prior in time, and the fathers of the council had only to apply to Jerome's case the norms of heresy worked out for Huss. In addition, Jerome was not a national leader as Huss had been; he had been out of Bohemia most of the time since about 1400.

One of Huss's most faithful followers was the cleric Peter Mladoňovice who accompanied the reformer from Prague to Constance and kept a detailed record of the trip and the trial. His *relatio* has been printed several times; the last version was a critical one by Václav Novotný, who added an edition of a contemporary German translation. This translation is based on Novotný's edition. Novotný regarded Peter's text as an effort to be fair to the reformer, but there is slight doubt that by modern standards Peter would have to be labeled somewhat nationalistic. His account presents Huss as grievously wronged by the council. So far as he goes, he incorporates many documents and adds color to the account by frequently reporting the atmosphere of the council.

Professor Spinka judged it helpful to present, as an introduction to the *relatio*, an account of Huss's life and thought prior to the council, emphasizing the doctrinal questions that led up to the actions at Constance. Such a confrontation—Huss versus the council—is all the more apropos in view of the recent appearance of two very substantial studies of Huss's thought by Dom de Vooght. Spinka demonstrates how De Vooght's understanding of Huss's doctrines differs from the conventional Roman Catholic position. Spinka's account occupies the whole first part of the book and brings the treatment available in Western languages up to date. He is thoroughly acquainted with the standard Czech scholarship; more recent Czech scholarship has scarcely been interested in doctrine, and its absence here is of little importance. It is now fair to say that the *relatio*, with all its detailed treatment of Huss's last months and the council itself, is more enlightening than before. A few slips, however, might be corrected in the event of a reprinting.

In addition to the annotated *relatio* and a richly informative introduction, Spinka translates a choice of letters relating to Huss's conflict with the establishment and his trial at Constance. Since Huss's letters were last translated into English by H. B. Workman and R. M. Pope in 1904, Spinka has done us all a great favor in providing the important researches of the last fifty years. An excellent bibliography makes this work a capital research tool.

University of Colorado

S. HARRISON THOMSON

THE LATER LOLLARDS, 1414-1520. By *John A. F. Thomson*. [Oxford Historical Series, Second Series.] (New York: Oxford University Press. 1965. Pp. 272. \$6.75.)

IN this volume Dr. Thomson deals with the history of Lollardry for a period not covered in other recent studies. After the failure of the Oldcastle rebellion in 1414 and the resulting widespread repression Lollardry continued to exist only in small communities, cutting across diocesan and county lines. Because of this lack of a common organization Thomson presents the history of fifteenth-century Lollardry in chapters showing the regional character of the surviving Lollard communities rather than on a chronological basis. These regions are all in the southern half of England; in the north and in Scotland there are few traces of Lollardry.

Basing his study on contemporary sources, utilizing particularly chancery,

judicial, and ecclesiastical records, printed or manuscript, the author gives detailed information about surviving groups and particular individuals within them and the forms that their heresies took. Not surprisingly, most of the information about the Lollards comes from the records of their trials for heresy. The procedure for investigating heresy was set forth in a statute of convocation of 1416, amended by later convocations and devised in accordance with the provisions of canon law. Although there were twenty-five major heresy prosecutions recorded between 1414 and 1522, the number of executions was not large and, interestingly enough, unorthodox thought was not suppressed. While beliefs varied from community to community and even among individuals within the same community, the author identifies two important aspects of Lollard doctrine: it was strongly antipapal, and it was puritanical. While the author does not attempt to deal with the influence of Lollardry on the English Reformation, he does affirm that "it was essentially a negative movement, opposed to the ecclesiastical establishment of its day."

The volume includes three helpful maps, a table of major heresy prosecutions recorded between 1414 and 1522, a bibliography of manuscript and printed sources and secondary authorities, and a useful index. Inevitably, because of the method of presentation chosen by the author, there is in this careful and scholarly study much repetitious detail, but this very detail supports overwhelmingly his own conclusion that, in spite of the number of trials and the concern of the ecclesiastical authorities, "there is little reason to believe, despite the abjurations, that the measures taken were having any real success in eradicating the Lollards."

William Woods College

ISABEL R. ABBOTT

DAS HAUSBUCH DER MENDELSCHEN ZWÖLFBRÜDERSTIFTUNG
ZU NÜRNBERG: DEUTSCHE HANDWERKERBILDER DES 15. UND
16. JAHRHUNDERTS. In two volumes. Edited by *Wilhelm Treue et al.*
(Munich: Bruckmann, 1965. Pp. 156; 275. DM 180.)

THE title of this work may be translated as the *House Roster of the Mendel Foundation for Twelve Fellows in Nuremberg*. The Mendels belonged to the sixteen oldest families of the imperial city's ruling patriciate. While Marquart (I) Mendel established a religious foundation in 1382, his brother, Conrad (I), displayed greater ingenuity. He endowed, six years later, a home for old men—the first such institution in Nuremberg, if not in Germany—which was to provide for the declining years of twelve "old, ill, and poor men" who were no longer able to carry out their respective crafts or trades. A fellow had to be a citizen of Nuremberg, of good repute, and a layman. Conrad was specific: no ecclesiastic was ever to be admitted as a fellow nor put in charge of his foundation. Conrad's grandson, Marquart (II), was no less original. As administrator of the foundation he had a pictorial house roster begun in about 1425.

Water-colored pen and ink drawings depict each fellow in the performance of his craft or trade. Legends indicate each inmate's name, profession, date of death, and, sometimes, additional information. The first of three volumes, the one covering the period 1425–1549 is reproduced here in its entirety. It is the only

known collection of this kind from the fifteenth century. Its uniqueness and, therefore, frequent examination and handling have made it necessary to repair, restore, and preserve the valuable illustrations; its reproduction is making it readily available without continued wear and tear on the originals. The 335 drawings in the volume represent 323 fellows and 12 administrators. The fact that almost 130 different occupations are listed and depicted speaks for the high degree of specialization, particularly in the handicrafts. The value of the illustrations does not rest on artistic merit, but on the clear, graphic display of tools, equipment, and simple machinery, of working conditions and procedures, of finished and semifinished products, and of costumes and furniture for an extended period of time. They offer information for historians interested in cultural, social, economic, and technological evolution.

The text volume contains, besides a facsimile reproduction of Conrad's deed and rules for the foundation, seven articles by experts illuminating from various angles the Mendel family, Conrad's foundation, and the house roster. Insufficient space makes it impossible to assess the sound individual contributions. One article, however, stands out: Wilhelm Treue and Rudolf Kellermann offer a brilliant and comprehensive essay on the social and economic significance of the Nuremberg handicrafts within the context of the complex economic ambitions and changing political configurations of the German city-states and territorial princes in the fifteenth and sixteenth centuries.

Polytechnic Institute of Brooklyn

FELIX F. STRAUSS

Modern Europe

THE EMERGENCE OF LIBERAL HUMANISM: AN INTELLECTUAL HISTORY OF WESTERN EUROPE. Volume I, FROM THE ITALIAN RENAISSANCE TO THE FRENCH REVOLUTION. By *Willson H. Coates et al.* (New York: McGraw-Hill Book Company. 1966. Pp. x, 357. \$6.95.)

Books on Western European intellectual history are coming thick and fast. No less than four have appeared in recent months if one includes, in addition to works by the present authors and Roland Stromberg, the second volume of John Herman Randall's *The Career of Philosophy* (which is much more than a history of philosophy) and the volume of essays entitled *Ideas in History*, edited by Richard Herr and Harold T. Parker, and also containing some useful comments on "the role of ideas in history." These follow by only a few years comprehensive books by Bronowski and Mazlish, and George Mosse, which were in turn slightly preceded by Crane Brinton's *Ideas and Men* (1950) and the first edition of my own *Main Currents of Western Thought* (1952). Since the publication of Randall's pioneering *The Making of the Modern Mind* in 1926, but particularly during the last twenty years or so, "Western" intellectual history, or the history of ideas as some still prefer to call it, has obviously come of age and grown very considerably in both popularity and sophistication.

Inevitably in the sort of publishing logjam that has now begun to develop, books tend to repeat each other somewhat and to go over a certain amount of familiar ground. Yet the subject is so vast that no two books are ever likely to come out the same. Each will have its own slant and develop its own themes in its own style. Messrs. Coates, White, and Schapiro, for instance, focus on the evolution of what they call "liberal humanism" which is said to constitute "the most important tradition of Western civilization." Wisely, however, they do not keep rigidly to this theme, for otherwise their book, which purports to be *An Intellectual History of Western Europe*, might seem rather one-sided and even perhaps a bit nineteenth century in conception. Timewise, it begins, rather abruptly I thought, with the Renaissance and ends with the French Revolution. The plan of organization calls for tracing major trends of thought—scientific, philosophic, and religious, as well as political, economic, and social—between these periods. Some attention is also paid to aesthetics.

The result is not only a broadly conceived but also a well-balanced and illuminating synthesis. Not the least of its virtues is its relating of intellectual to other kinds of history, of ideas to a wide context of historical events. It lacks, and, in my opinion, needs, a good definition of "liberal humanism," as also perhaps "modern mind" or "modern Western outlook" which becomes increasingly conspicuous in the text from the eighteenth century on. But perhaps the full meaning of such key terms will emerge in the second volume of this work, which promises to continue the story with special emphasis on "the ordeal of liberal humanism" in the nineteenth and twentieth centuries.

Yale University

FRANKLIN L. BAUMER

GUNS, SAILS AND EMPIRES: TECHNOLOGICAL INNOVATION AND THE EARLY PHASES OF EUROPEAN EXPANSION 1400-1700. By Carlo M. Cipolla. [Pantheon Studies in Social History.] (New York: Pantheon Books. 1965. Pp. 192. \$5.00.)

"Religion supplied the pretext and gold the motive. The technological progress accomplished by Atlantic Europe during the fourteenth and fifteenth centuries provided the means." In this technological progress two lines of advance were of peculiar and overwhelming significance: the design and construction of sailing ships and the manufacture and tactical use of firearms, especially of ship-borne artillery. So far, Professor Cipolla's explanation of European successes outside Europe, in what he calls the "era of Vasco da Gama," is a familiar one, and a number of books propounding much the same thesis, though with variations of emphasis, have appeared in recent years. In one respect, however, Cipolla takes the argument further. He gives a fascinating summary of the history of the development of gun founding in Europe in the fifteenth and sixteenth centuries. He shows how the structural weakness of forged iron guns and the technical difficulty of making large iron castings of appropriate design led to a preference for copper alloys, despite their high cost. He traces the intimate connection between the skills of gun founding and bell founding and emphasizes the immense

advantages conferred by easy access to supplies of copper and tin. It might, perhaps, have been slightly more accurate to write of brass guns rather than bronze—most sixteenth-century recipes for gun metal prescribed an admixture of “latten” or zinc—but the metallurgical terms of the time were themselves imprecise. In the later sixteenth century improvements in casting methods, especially in England, brought iron guns back into favor. Cipolla shows how in these specialized developments the Asian peoples fell further and further behind.

Cipolla treats his sails more cursorily than his guns. Apart from the truism that galleys are unsuitable for long ocean passages, we are given no clear explanation of why Atlantic Coast ships were superior to Mediterranean in the fifteenth and sixteenth centuries, or why European ships were superior to Asiatic ones. The technical achievements of the Venetians and of the Chinese are underrated, and, within Europe, the crucial question of the availability of materials is largely ignored. Finally it may be questioned whether in the sixteenth century, or even the seventeenth, European domination of Eastern waters was as complete, or as rapidly established, as the author suggests. In well-founded, well-armed ships, Europeans could certainly pass unchallenged (except by one another) wherever they liked on the high seas, but surely it is an exaggeration to state that “Within fifteen years after their first arrival in Indian waters the Portuguese had completely destroyed the naval power of the Arabs and their King could *justifiably* [*italics mine*] style himself ‘Lord of the Conquest, Navigation and Commerce of Ethiopia, Arabia, Persia and India.’”

Johns Hopkins University

J. H. PARRY

THE HISTORY MAKERS: THE PRESS OF EUROPE FROM ITS BEGINNINGS THROUGH 1965. By *Kenneth E. Olson*. (Baton Rouge: Louisiana State University Press. 1966. Pp. xiii, 471. \$10.00.)

KENNETH Olson has written the only usable history of European newspapers in English. This study deals with the part the press “has played in the political, economic, and cultural development of their peoples, and also the . . . men who have made this press.” The story, moreover, “can only be explained against the backdrop of the historical, political, economic, and cultural development of the people of each nation. This book is that explanation.”

It is difficult to write the history of European newspapers, and it is made harder by filling in what the author calls the “backdrop.” The result is a book that does not decide whether it is for the scholar or the general reader. The historical background that the author provides is generally accurate but, at least where major nations are concerned, unrewarding. We are provided with much information that is neither new nor welcome. Sometimes the book becomes irrelevant.

What repeatedly unsettles the reader, just when he is regaining his confidence in the work, is the shaky grasp of history frequently demonstrated. For example, eighteenth-century English-language newspapers, we are told, were an advertising medium aimed at the workers in the colonies who were attracted to the “cities” by the Industrial Revolution. And in the chapter on Spain the reign of Isabella II is described as “despotic,” the role of the Falange before the Civil War

is exaggerated, Franco's dictatorship is called a "fascist state," and "the church and all the clergy were back of Franco."

This kind of talk is disheartening enough. But it is most disappointing not to find an attempt to tell what freedom has meant to Europeans at various times and whether the notions embodied in constitutions represented a consensus and were respected. One may speak of efforts by the popes "to keep the people in ignorance," but one must go on from there.

It is to the author's credit that he did not exclude even the smallest European nation and that the anecdotes enhance his engaging style. The book is especially valuable for its summary of today's European newspapers. We do not see clearly what the great figures of European newspapers were like, nor are we told why their papers became great or declined, but the work includes all of them handily in a single volume.

Washington, D. C.

JOHN ADAM MOREAU

THOMAS MORE AND ERASMUS. By E. E. Reynolds. (New York: Fordham University Press. 1965. Pp. x, 260. \$6.00.)

Mr. Reynolds is a well-known Catholic biographer of Thomas More, his family, and his circle, and the author, most recently, of the well-received *The Trial of St. Thomas More*. Unlike this closely documented, factual account of a specific dramatic event, *Thomas More and Erasmus* is a broad work of interpretation; or it should be. For it is at precisely this crucial point that it must be most seriously criticized. The reader has every reason to expect a searching study of the effect of one man upon the other. Nothing else would give the book much reason for having been written. Instead, what we have are two parallel biographical sketches written in alternating chapters and dealing almost entirely with the externals of their relationship. The book is descriptive rather than either interpretive or analytical. Indeed, if one were not already aware of the fame of Erasmus and More, it would be hard to determine why their friendship would justify even this small book and the reader's time.

This point raises another question: for whom was the book written? Presumably, for the interested general reader. Reynolds declares in his preface that he has "preferred not to pepper the pages with references." While one cannot quarrel with his decision, one may object to the capriciousness with which he has used his pepper. For example, he makes heavy use of Erasmus' letters and cites as the general source Allen's *Opus Epistolarum*. Yet the Nichols translation of *The Epistles of Erasmus*, to which most of the quoted passages are obviously indebted, is not mentioned at all. Repeatedly Reynolds writes right across prickly points of controversy with no reference to the scholarship or even, in most cases, to the fact that controversy exists. At other places he distracts his reader with completely irrelevant footnotes. While many of the standard modern critical works find their way into the notes, one has an uncomfortable feeling that they have had little effect upon the body of the text. Huizinga, Renaudet, and Phillips are barely mentioned; Chambers, Reedijk, Caspari, and Surtz, not at all.

Finally, the work is flawed by a succession of annoying small errors of fact and gratuitous judgments at variance with what scholarly consensus does exist. In all it is a book not quite adequate to the promise of its subject

Wichita State University

J. K. SOWARDS

STOLBOVSKII MIR 1617 G. I TORGOVYE OTNOSHENIIA ROSSII SO SHVEDSKIM GOSUDARSTVOM [The Peace of Stolbovo of 1617 and Trade Relationships between Russia and Sweden]. By I. P. Shaskol'skii. (Moscow: Izdatel'stvo "Nauka." 1964. Pp. 215.)

THIS book covers some of the results of a long chapter in Russian history that began with the invasion of the Baltic area by Ivan the Terrible and continued with the involvement of many European powers in Baltic affairs, the Time of Troubles in Russia, and that country's subsequent partial invasion by foreign powers.

The Treaty of Stolbovo temporarily ended a prolonged struggle between Russia and Sweden and created the basis for peaceful trade relations between the two countries. The treaty, no doubt, was more favorable to Sweden than Russia, and Sweden attempted to play the role of a middleman in trade relations between Russia and the West. In I. P. Shaskol'skii's book the Swedes do not play the role of "good guys," as they are customarily depicted, but rather of selfish people bent on the exploitation of others.

In this book the author writes more like an ardent Russian patriot than a Marxist-Leninist. He prefers Russian domination of the Baltic area to international cooperation. In order to justify the invasion of the Baltic by Ivan IV and his predecessors, he borrows the Stalinist formula of the capitalist encirclement of the Soviet Union and applies it to feudal Russia, surrounded by vicious little neighbors all allied and bent on terrible exploitation and destruction of the Russian giant. The author overlooks the fact that the small Northern and Central European powers were engaged in almost perpetual warfare among themselves and that all of them were deadly fearful of their huge Russian neighbor notorious for its semibarbaric and cruel governmental system and for its destructive military campaigns in neighboring countries.

In Shaskol'skii's treatment of big-power politics the Baltic nations themselves are practically nonexistent: their feelings, interests, and sufferings at the hands of all kinds of invaders are of no concern to the author. That they would prefer an independent, undisturbed life and cooperation with all their neighbors does not occur to many historians who cover wars in the Baltic.

A careful reader will find Shaskol'skii's book valuable despite its shortcomings. It is based on many Russian archival sources practically unavailable to Western scholars. It also demonstrates generous use of Swedish and Finnish sources and some use of German, French, British, and Estonian materials. The footnotes are extensive and highly satisfactory. There are also interesting enclosures, but regrettably there is no bibliography.

The author's approach is frankly that of a Russian imperialist and materialist of the old Stalinist school, but there is a marked difference between him and the

dilettanti of the preceding decades. Shaskol'skii is a scholar, and his footnotes are not cluttered with references to completely irrelevant works by Lenin, Stalin, and so forth, and quotations from such works. I find this book in many respects highly enlightening and a valuable contribution to a half-forgotten field.

San Jose State College

EDGAR ANDERSON

SOIXANTE-DIX-SEPT LETTRES INÉDITES À NICOLAS HEINSIUS (1649-1658). By *Jean Chapelain*. Published after the manuscript at Leiden with an introduction and notes by *Bernard Bray*. [Archives internationales d'histoire des idées, Number 13.] (The Hague: Martinus Nijhoff. 1966. Pp. viii, 467. 50 gl.)

THE seventy-seven hitherto unpublished autographed letters of Jean Chapelain to Nicolaas Heinsius in this handsome volume fill a large gap in the life of the French critic, poet, and early member of the *Académie française*.

The standard collection of Chapelain's extensive correspondence is that edited by Tamizey de Larroque in the "Collection des documents inédits sur l'histoire de France" (1880, 1883). It consists of about 2,500 pieces, some reproduced in full, but most simply catalogued. Unfortunately, the entire period 1641-1658 is unavoidably blank in Tamizey's edition. The letters in this volume thus present welcome information on Chapelain's activities and thought in a period for which little firsthand information has been available. These letters represent perhaps a third of the known correspondence between Chapelain and Heinsius; the remainder (for the period after 1658) may be found in Tamizey's volumes. Precisely why the long and cordial exchange between the two men was begun in 1649, under Chapelain's initiative, is not clear. The Frenchman was senior to Heinsius by twenty-five years and at mid-century enjoyed a reputation in European letters far greater than that of his young Dutch correspondent, although the latter was clearly showing promise of following in the footsteps of his illustrious humanist father, Daniel Heinsius. Chapelain was apparently interested in the epistolary style for its own sake and saw an opportunity to launch a correspondence in the grand tradition of classical and Renaissance writers. He was not disappointed; for more than two decades the two men averaged almost monthly exchanges of views. Regrettably, an unwritten rule of *le style épistolaire*, as Chapelain and Heinsius interpreted it, was that they refrain from giving news of mundane events. Their literary views and preoccupations, their reflections on morals, their personal activities, even their state of health, were all fit subjects for the *lettre familière*, but, at least theoretically, they disdained the role of reporters or commentators on events of historical note. Chapelain's two dozen or so letters written in the midst of the *Fronde*, for example, reveal relatively little information on the stirring events taking place about him.

The editor, Bernard Bray, has provided a long introduction for the letters considered as literature, along with excellent annotations (mostly historical in nature) of the letters proper.

University of Notre Dame

LEON BERNARD

ASPECTS OF THE EIGHTEENTH CENTURY. Edited by *Earl R. Wasserman*. (Baltimore: Johns Hopkins Press. 1965. Pp. vi, 346. \$7.00.)

THIS volume contains a series of papers presented at the Johns Hopkins University's 1963 postdoctoral seminar devoted to the eighteenth century. The lectures were given by historians, philosophers, musicologists, art historians, and students of literature; the resulting volume is therefore somewhat difficult for any one scholar to review, since few of us can claim any real competence in so many disciplines. I found considerable empathy with the perceptive essay by George Boas, "In Search of the Age of Reason," because his analysis beautifully erodes the traditional stereotyped nineteenth-century notions about "ages" and "periods" without becoming mere iconoclasm. Isaiah Berlin's "Herder and the Enlightenment" also interests the historian; his Herder is both plausible and significant as a thinker, but, more important, he is a scholar and teacher whose ideas emerge from his researches and reflections. He is also more than a precursor of nineteenth-century nationalism. Henry Guerlac's essay, "Where the Statue Stood: Divergent Loyalties to Newton in the Eighteenth Century," is a nice analysis of the variety and the conflicts within the interpretations of the Newtonian system that gives nominalistic meaning to the process of acclimatization of the scientific revolutions of the seventeenth and eighteenth centuries. It is a useful antidote to the realism of Cassirer. Guerlac shows us clearly that there were several branches of the Newtonian tradition, each with its own advocates and its own achievements. Alfred Cobban produces an interesting and provocative essay, useful to the historian. In his "The Enlightenment and the French Revolution," he sees the Enlightenment's main line of development moving toward a utilitarian point of view in which men, ideas, movements, and causes would stand or fall by the criteria of their usefulness to men on this earth. In a sense this is to say that the Lockean traditional liberalism was the center of the movement we call the Enlightenment. Cobban argues that the French revolutionary ideologists started in this tradition: their reform of the laws and court procedures, their toleration, their abolition of slavery and the slave trade, their educational reforms were all in the spirit of utilitarian ideals. But, he goes on to argue, Robespierre's Republic of Virtue, the movement toward government by an elite, the crusade to rebuild Europe on the French model, and finally the rise of Napoleon to power tore the Revolution away from its original roots and distorted the movement. As a good Englishman, Cobban finds in Bentham and his school the "true" heirs of the eighteenth-century Enlightenment. The thesis is interestingly propounded, but it does leave unanswered a few questions about both the Enlightenment and its heirs.

Other readers of this book will not find esoteric or jejune the essays by literary critics, art historians, musicologists, and philosophers, but I, in all honesty, must disclaim competency to judge most of them. Perhaps I can do my duty if I list them: J. A. Passmore, "The Malleability of Man in Eighteenth-Century Thought"; René Wellek, "The Term and Concept of 'Classicism' in Literary History"; Jean Seznec, "Diderot and Historical Painting"; R. Wittkower, "Imitation, Eclecticism, and Genius"; Edward E. Lowinsky, "Taste, Style, and Ide-

ology in Eighteenth-Century Music"; Maynard Mack, "*Secretum Iter*: Some Uses of Retirement Literature in the Poetry of Pope"; W. J. Bate, "The English Poet and the Burden of the Past, 1660-1820"; Georges May, "The Influence of English Fiction on the French Mid-Eighteenth-Century Novel"; and Heinz Politzer, "The Tree of Knowledge and the Sin of Science: Vegetation Symbols in Goethe's *Faust*." The lecture series must have been exciting for those who attended; the published essays provide a provocative and somewhat varied picture of the eighteenth century.

University of Illinois, Chicago Circle

JOHN B. WOLF

THE ROAD TO SARAJEVO. By *Vladimir Dedijer*. (New York: Simon and Schuster. 1966. Pp. 550. \$11.95.)

EVER since June 28, 1914, when nineteen-year-old Gavrilo Princip killed the Habsburg heir, Archduke Franz Ferdinand, in Sarajevo, the issue of responsibility for World War I has remained an unresolved although fundamental question, for its presumed answer provided the cornerstone of the world created at Versailles.

Now, over half a century since Sarajevo, a comprehensive and dispassionate study of this issue is at last available. *The Road to Sarajevo* explores in depth virtually all accessible archival materials; it surveys and analyzes a broad spectrum of sources and examines major hypotheses, charges, and countercharges; and it projects facts against the background of cultural variants, doctrinal influences, traditions, and passions—the context of the motive forces of spirit that render historical records intelligible. It may be the definitive work on the subject.

The complex image of two juxtaposed worlds on the eve of the great conflagration is re-created with clarity, with sharply etched lines of domestic and international affairs. One is the world of an aging empire whose relative prosperity, fictitious stability, and cultural veneer obscure its corrupt legalism, imperious power politics, and underlying occasional barbarism. The other is a world of dynamic nationalism replacing despair by wild expectations as independence is equated with justice and political morality whose values are sought equally in ancient ballads and in modern social doctrines. The two protagonists, the middle-aged Archduke and the young Princip, personify the two worlds.

The Archduke, stout, pompous, arrogant, and biased, is personally a man injured by illness and by the imperial court's contempt toward his wife. He finds his outlet in dreams of highhanded restoration of his dynasty's decayed grandeur as much as in hatred of Magyars, Jews, parliamentarians, all liberals, and, above all, Serbia. His archaic prejudices blend with his fears of the present and future. Princip, youthful and taciturn, is a peasant's son struggling for an education, a voracious reader who lives by the Serbian legend of Kosovo and its values of self-sacrifice for moral ends, while, with his Young Bosnian schoolmates, he searches for a doctrine by which to save the world. Contrasting free Serbia's democracy with the Dual Monarchy's inequities, its unilateral annexation of Bosnia in 1908, and its preservation there of the absurdly oppressive Ottoman system of serfdom, he sees tyrannicide in terms of poetic justice.

Step by step, the author brings the two protagonists to their fateful encounter on St. Vitus Day, the 525th anniversary of the Kosovo battle where Serbia fell to the Turks—a date believed by some as chosen by the Archduke in order to add insult to the injuries suffered by the Bosnians. Attempting desperately to deny Vienna a *casus belli*, Serbia warns Austria of the imminent danger and seeks unsuccessfully to stop the Young Bosnians. While too many in positions high and low wish for the death of what the Archduke personifies, his war party is too eager for a *casus belli* to provide effective security for him. Despite the failure of the Young Bosnians' dilettante plans, he dies through an odd combination of coincidences and a schoolboy's aroused sense of justice. The flames fanned from a spark by irresponsible men consume an age made combustible by the injustice of some and the complacency of too many.

Long Island University

DRAGOŠ D. KOSTICH

THE FOUR POWER PACT, 1933. By *Konrad Hugo Jarausch*. (Madison: State Historical Society of Wisconsin for the Department of History, University of Wisconsin. 1965. Pp. viii, 265. \$4.00.)

THE concert of Europe, a loose yet not ineffectual reality, was the nineteenth-century answer to the problem of how a collectivity of sovereigns could operate in an orderly way. After the failure of the concert, the League was an attempt to find a more dependable system. But the League proved incapable of solving many problems; it failed to reconcile the equally legitimate French claim to security with the German one to equality.

The conjunction of the stalemate in the disarmament discussions with the advent of Nazism to power was adequate reason for alternative proposals. This is the genesis of Mussolini's scheme of a Four-Power Pact, a solution in itself not devoid of merit. It also fitted well into the promotion of the Italian interest, traditionally best enhanced through exploitation of the equilibrium of power. Not surprisingly, the suggestion appealed to the British, but was suspected by the French and the Germans. "London and Rome were still close enough to each other to mediate between Paris and Berlin" is aptly put; the mediation was successful to the extent that the Four-Power Pact was signed in Rome in June 1933.

But the instrument that was signed bore little resemblance to the initial proposal of March. For "if Mussolini's aim had been to calm Europe's nerves, he achieved the very opposite result." Whatever its merits may have been, the scheme obviously was an alternative to the existing power structure; France and particularly its clients were justifiably alarmed. If the latter, Poland excepted, were eventually pacified, it was because diplomacy, French diplomacy above all, performed in the interval a skillful job of emasculation. What had started as an alternative to the League ended largely as a reassertion of devotion to that failing institution. The Four-Power Pact had no significance or influence, save as a warning of future possibilities. The worst fears of its opponents were realized within five years.

The origins of the proposal and the subsequent wearisome negotiations are

carefully and competently traced in this monograph. The concluding analysis in particular is a judicious assessment of an episode in the continuing story of how powers endeavor to deal with each other.

Barnard College

RENÉ ALBRECHT-CARRIÉ

ZSSR A GRANICA NA ODRZE I NYSIE ŁUŻYCKIEJ, 1941-1945 [The USSR and the Boundary on the Oder and Lusatian Neisse Rivers, 1941-1945]. By *Włodzimierz T. Kowalski*. (Warsaw: Wydawnictwo Ministerstwa Obrony Narodowej, 1965. Pp. 279. Zł. 29.)

WŁODZIMIERZ T. KOWALSKI is a young historian on the research staff of the Polish Institute of International Affairs. His book was preceded by several articles on the genesis of the Oder-Neisse Line and represents the first historical monograph on the subject to be published in Poland.

The author's stated objective is to demonstrate the "distorted" approach of many Western historians, especially those in West Germany, who isolate the Oder-Neisse question from its context in the development of a general security system for postwar Europe and who view it chiefly in terms of "compensation" for the loss of Poland's eastern territories to the USSR. He challenges the claim that the new frontier and the deportation of the German population was the sole responsibility of the Soviet Union. Finally, in tracing the genesis and evolution of the problem, he stresses the contribution of Polish political thought as embodied in the programs and influence of the Polish Workers' party, the Union of Polish Patriots, and the Polish Committee of National Liberation.

Kowalski is certainly right in stressing that both Roosevelt and Churchill accepted the Oder as Poland's western frontier as early as 1943, and also that they accepted the principle of a transfer of population. Differences arose later on the subject of the western or Lusatian Neisse, which British and United States statesmen opposed for political and not for humanitarian reasons. By that time Soviet and European security had ceased to be synonymous. The author fails to convince the reader that the Polish Communists exerted an influence on the establishment of the Oder-Neisse boundary. By Kowalski's own admission, Stalin proposed the Oder to Sikorski as early as December 1941, before either the Polish Workers' party or the Union of Polish Patriots had come into existence. He also fails to provide evidence for his assertion that the Polish Communists played an important role in Stalin's demand for the western Neisse. He contributes nothing new on Soviet policy while his presentation of the policies of the Polish *émigré* governments and those of the United States and Great Britain is rather one-sided. The book lacks a bibliography and an index, but includes some unpublished Polish documents of interest.

University of Kansas

ANNA M. CIENCIALA

COMMUNISM IN EUROPE: CONTINUITY, CHANGE, AND THE SINO-SOVIET DISPUTE. Volume II. Edited by *William E. Griffith*. [Center for International Studies, Massachusetts Institute of Technology. Studies in International Communism, Number 6.] (Cambridge, Mass.: M.I.T. Press, the Institute. 1966. Pp. xiv, 439. \$12.50.)

THIS second volume of a series examining the evolution of European Communist parties and the impact of the Sino-Soviet conflict on them continues the painstaking scrutiny begun in Volume I in regard to Yugoslavia, Poland, Hungary, and Italy. It contains an introductory survey, "European Communism, 1965," by William Griffith; sizable histories of the Communist parties of East Germany by Carola Stern, and of Czechoslovakia by Z. Eliáš and J. Netík, which are welcome additions to the sparse literature on the subject; and briefer treatments of those of Sweden, Norway, and Finland.

Intended to assess the "rush of change" in European Communism, the series is caught in it. As Griffith observes, the categories of "dogmatic" and "revisionist" parties used in the first volume (published in 1964) were no longer meaningful in November 1965, when the second volume was completed. Creative development of Marxism is the order of the day everywhere, and dogmatism is a pejorative no European Communist—the Albanians excepted—would wish to deserve today. Along with fluidity in ideology, there are other potent factors for change at work: the rapid and extensive decline of Soviet prestige, influence, and authority; the resurgent nationalism within and without the Communist parties; the Sino-Soviet rivalry for leadership in the world Communist movement. The effects of the last, however, are elusive for the contemporary historian.

Like the floating iceberg, even in fog-free conditions only a small part of the evidence of contemporary history is visible. And Communist affairs are hardly free of fog. It is perhaps the problem of evidence that accounts for the omission of Bulgaria, where the factors for change have also been at work: Bulgarian dogmatists have in recent years been thoroughly discredited; nationalism is making itself increasingly vocal; and the impact of the Sino-Soviet rivalry has been felt in such developments as the Bulgarian "great leap forward" in 1959 and the abortive *coup d'état* in 1965. Griffith and his associates are, nonetheless, to be commended for producing important and reliable pieces of current history, based on high standards of scholarship, clinical precision, and scrupulous search for facts. Griffith is also to be commended for providing a fine example of what Western scholarship is supremely capable of doing, but has not practiced often enough: bringing talents together across boundaries and oceans and using the best brains wherever they can be found.

San Fernando Valley State College

MARIN PUNDEFF

THE HABIT OF AUTHORITY: PATERNALISM IN BRITISH HISTORY.

By *A. P. Thornton*. (Toronto: University of Toronto Press. 1966. Pp. 402. \$7.50.)

A. P. THORNTON sets himself a formidable task in this work; his concern is to describe the nature of British politics and society since the Norman Conquest.

Wisely, he chooses to concentrate on the last two centuries. In paternalism—the willingness of a minority to accept and wield authority and the readiness of the majority to accept and support that solution—Thornton finds the key to much that is characteristic of British culture. His analysis affords new insight into the qualities of class relations in Britain; it goes far to explain why liberty and not equality was the conventional demand and why “tradition” should have been the cloak chosen even by so-called “revolutionary” movements. Thornton recognizes that Toryism was never the philosophy of a single party, but that its conservative preferences insinuated themselves everywhere, making moderation and subordination seem natural and appropriate responses to the exercise of authority. The system’s safety valve was its capacity to tolerate new “ascendancies”; these, however, invariably perpetuated the idea of a hierarchical relation.

It is impossible to read this book without recognizing the author’s intellectual indebtedness to Walter Bagehot. Thornton’s terminology is different from Bagehot’s, but the ideas are often the same. If Bagehot distinguished between “clever people” and “stupid ones who mind their business and have a business to mind,” it was because he knew that society was maintained “by dull care” and “by stupid industry.” Thornton values less than Bagehot the traditional British distrust of the intellectual, but accepts the fact that “England continued to react far more strongly to events than to ideas.” The British understood “the idea of aristocracy”; they were less comprehending of abstractions like “social efficiency” and “social justice.”

Thornton’s argument emphasizes the continuity of British development; it suggests that there were no important breaks, that neither the Industrial Revolution, the political enfranchisement of the middle classes, nor the building of a great empire constituted a genuine departure from the traditions of subservience to paternalist practices. The author’s interest is to explain why British society was so close knit; why, despite class differences, it possessed a “national outlook,” of the kind T. S. Eliot described in his *Notes towards the Definition of Culture*. Many historians—and not only those who accept the conclusions of Hobsbawm—will raise questions about this so-called unity.

Bagehot worried lest “deferential attitudes” should be destroyed by the political reforms of 1867. He did not live long enough to have his fears confirmed, but their intensity is suggested by his 1872 introduction to *The English Constitution*. Thornton, writing from the perspective of the last third of the twentieth century, implies that the domination in fact did not end. In the chapter entitled “Bonar Law’s Dynasty,” he surveys the thirty-five years of “paternal rule”—essentially Tory rule—of the period 1910–1945. World War I gave a severe shock to the old ideas; it made men doubt the wisdom of their “governors,” but this was not sufficient to undermine their position. Thornton asks whether paternalism is dead even today. The “rule of meritocracy” may simply be the final chapter in the history of paternal government. Many will wish to argue with this book; few will choose to ignore it.

Brown University

STEPHEN R. GRAUBARD

BOOKS TO BUILD AN EMPIRE: A BIBLIOGRAPHICAL HISTORY OF ENGLISH OVERSEAS INTERESTS TO 1620. By *John Parker*. (Amsterdam: N. Israel. 1965. Pp. viii, 290. \$8.50.)

HISTORIANS are becoming increasingly aware of their indebtedness to bibliographers and librarians. Indeed, two former associates of the late A. S. W. Rosenbach—Edwin Wolf II and E. Millicent Sowerby—immediately come to mind to illustrate the unique scholarship to be contributed by the trained bibliographer. It is largely owing to such library-oriented scholars that we are beginning to appreciate the significance of books as a species of source material, as a window on the intellectual origins of a man or an era. John Parker thus joins a select circle with this work. Trained in library science, he examines the role of the English presses in stimulating and feeding English interest in imperial adventure.

Parker's avowed objective is, to be sure, rather carefully delimited. He omits, purposefully, works on the science of navigation and related areas, because a study is already under way on such books. He also excludes travel narratives and geographies relating solely to Europe. He sets his sights firmly on some 267 titles and editions issued in England between 1481 and 1620, books that reflect and inform on the mounting English interest in empire in this period.

I sympathize with Parker's thesis and find much of value in his book. It is useful to be reminded of the mushrooming English enthusiasm for the printed page, an interest, however, that lagged behind that of the Dutch. Few can quarrel with so convenient a survey of English publishing. Disappointment and frustration come when one searches for evidence of the impact of such books on the rather narrow market afforded by literate Englishmen in the sixteenth century. We find no concern for the ideas transmitted by such books, no curiosity as to their intellectual consequences. The mere fact of publication has its significance, of course; success and possible influence can be suggested by the number of editions a work enjoys. But Parker musters little evidence to support his claims for "fundamental importance." He casts too little light on the English literary and publishing scene that produced his books; he is too unconcerned with the response stimulated in public men, men of power and influence in the history of exploration and colonization.

There is no denying the difficulties confronting this study. Parker deserves commendation for his sometimes extravagant survey of English exploration in the sixteenth century and for his important bibliography, but his book remains too much of a survey. And I, at least, am surprised at the author's failure to explore the unique holdings of the Library Company of Philadelphia.

Indiana University

TREVOR COLBOURN

POLITICS AND PROFIT: A STUDY OF SIR RALPH SADLER, 1507-1547. By *Arthur Joseph Slavin*. (New York: Cambridge University Press. 1966. Pp. xvi, 237. \$10.00.)

THIS monograph is a welcome addition to the jigsaw of the Tudor portrait. Ralph Sadler, knight, businessman, and principal Secretary of State to Henry

VIII, held many public offices in a life of service to the Tudors that extended well into the reign of Elizabeth I; he was one of the first modern "civil servants." His life, business ventures, and civil career have never been subjected to a thoroughgoing scholarly investigation—a lack consistently frustrating to students of the period since his name appears so frequently in both contemporary records and later historical literature. Professor Slavin's brief work covers only (after initially tracing Sadler's heretofore obscure early years) the period of his myriad activities in behalf of the second Tudor. His facile integration of ancient evidences with the product of recent scholarship in this study is admirable.

Slavin's fundamentally significant contribution in this scholarly and closely documented partial biography is, however, not merely in his precise exposure of the life and public services of a single royal agent who never really attained the higher echelon of royal service. Sadler was first Thomas Cromwell's man, and later Henry's, and the author skillfully exploits his subject's relations with the great and near great as a vehicle for entry into the social, economic, and political premises of the Tudor cosmos, and consequently into the philosophical principles that seem to have made them applicable to the often brutal realities of that world. This particular contribution is best exemplified in the volume's eighth and ninth chapters, "Place and Profit: The Exploitation of Office," and "Place and Profit: Sadler and the Henrician Land Market," which, while of considerable value to the scholar, will prove worth a half-dozen lectures to the teacher attempting to revitalize the spirit of a long-gone age.

The misdemeanors that mar this volume are literary in nature and petty in degree. The superabundance of textual superior arabics and their dependent citations, some of which appear of questionable necessity, too often disrupt the concentration demanded by the quality of the materials. The same zealotry for spearing facts applied to the general index would have strengthened that part of the work. Not all readers will agree with the author's decision not to normalize spelling and punctuation in textual and citational quotations, although exact rendition does retain something of the Tudor flavor.

DePaul University

PAUL L. HUGHES

SCOTLAND: JAMES V TO JAMES VII. By *Gordon Donaldson*. [The Edinburgh History of Scotland, Volume III.] (New York: Frederick A. Praeger. 1966. Pp. x, 449. \$12.00.)

A NEW, detailed, general history of Scotland has long been needed; none has been written since the beginning of this century. As the jacket blurb for the present volume accurately remarks, therefore, "The four-volume *Edinburgh History of Scotland* . . . is the most important project in Scottish historical writing for more than half a century." If the other volumes maintain the standards of this one, the project will be not only important but also superlatively executed.

Professor Donaldson divides his work into three main sections: before, during, and after the fifty-eight-year rule of James VI. By contrast with those scholars who tend to think of this king as James I and a failure, Donaldson praises him highly for his political intelligence, logic, and common sense, and regards him

rightly as the most successful of the Stewarts. He was able to gain control of his fractious aristocracy and his God-intoxicated clergy, and he kept his kingdom at peace; the disasters that overtook the Scottish crown after his death were owing to the ignorance and tactical blunders of Charles I. Donaldson is also sharply critical of James V, whose combination of venality and sadistic cruelty ended by alienating the political classes. He was, says Donaldson, "perhaps a Tudor rather than a Stewart in character." Donaldson does not admire the Tudors; he is particularly critical of Elizabeth, whose reign "ended in anti-climax, in decline, almost in failure."

Donaldson is best known for his work on church history of the sixteenth and seventeenth centuries, and, inevitably, he has much to say about this subject in these pages. His account of the Reformation and its consequences, especially in the matters of church government and finance, is judicious, well balanced, and clear. He convincingly reiterates his view that the episcopal solution devised by James VI and restored after 1660 had a good chance to succeed, that the triumph of Presbyterianism was by no means inevitable, and that it was the blunders of Charles I and of James VII that wrecked the chances of Scottish episcopacy. There are also excellent accounts of the structure of society, economic and cultural life, and the constitution. This last chapter is limited to a discussion of the central government; some account of the operations of local government would have been useful.

It is impossible in a short review to do full justice to the many merits of this book: the stylistic grace and wit, the profound scholarship, the many originalities of interpretation on specific points, the clarity and balance of the narrative. Not everyone will agree with all of Donaldson's views, but that is a small matter. This is a really splendid book, which will remain the standard account for many years.

Rutgers University

MAURICE LEE, JR.

WILLIAM LAMBARDE, ELIZABETHAN JURIST, 1536-1601. By *Wilbur Dunkel*. (New Brunswick, N. J.: Rutgers University Press. 1965. Pp. xxiv, 210. \$7.50.)

For the many lines of historical and practical scholarship he initiated in the years of Elizabeth I, William Lambarde has long deserved a book-length biography like this. In the sentimental light of his final interview with his Queen, which he himself recorded so engagingly, he is very much the model of the retiring and devoted scholar-servant of his country, and librarians and researchers have long been accumulating whatever scraps of evidence turn up on his life. It is such fragmentary evidence, particularly from the Folger Library's collection of manuscripts, that Professor Dunkel has valiantly attempted to turn into a rounded account of a significant career.

Often the data are still too scanty, however, and the speculation resorted to seems forced. Data are most plentiful on Lambarde's activities as a leading justice of the peace in Kent from 1580 to 1600. Dunkel recites all of it as evidence that Lambarde was something of an imposing figure, and wonders almost with in-

dignation why Lambarde was not knighted. Conyers Read, in editing the same records of activities and speeches for the Folger Library, perhaps had a sharper sense of reality when he concluded that Lambarde was "a kind of *bonne à tout faire* for the Crown in Kent." It is a bit hard, also, to join Dunkel in seeing, in Lambarde's earnest rhetoric to juries, evidences of an exciting democratic view of the common man.

Unquestionably many further manuscript traces of Lambarde remain scattered in private hands and in English libraries. Speaking of the *Archaionomia*, Dunkel acknowledges that he is something of an outsider to historians' technical discussions. A notable omission, all the same, is any mention of Lambarde's parliamentary precedent book of about 1587, which Sir John Neale has so highlighted. An equally surprising inclusion is a peculiar letter of July 1585 to Lord Burghley, which the author treats confidently as a unique revelation of Lambarde's views and relationships even though, as Read noted, it was almost certainly written by someone other than Lambarde, someone who had much more intimate connections with the government. Moreover, Dunkel repeatedly stresses the importance and influence of Lambarde's *Archeion* and regrets that it was withheld from publication by Sir Robert Cecil; yet he does not mention its revision in 1596 and apparent presentation, at that time, to Lord Keeper Egerton.

But it is pleasant to have a number of scattered letters and episodes fitted into the Lambarde story, as steps toward completing the jigsaw puzzle. The book has as a most charming introduction Mrs. Bowen's essay on "Historians Courageous" for the American Philosophical Society (1957). Though this gives a date for the beginnings of the Society of Antiquaries that we now know to be unsatisfactory, and though it may rouse false hopes in the reader that something will be said about Lambarde's relation to that society, it provides for the biography as a whole the more suitable larger perspective.

Washington, D. C.

PAUL L. WARD

THE EARLY ELIZABETHAN SUCCESSION QUESTION, 1558-1568. By Mortimer Levine. (Stanford, Calif.: Stanford University Press. 1966. Pp. viii, 245. \$6.95.)

THIS tightly organized and well-written monograph will become the definitive work on early Elizabethan succession. Dr. Levine has thoroughly examined all aspects of the question, going beyond parliamentary speeches and proceedings to evaluate tract literature. It was not his intention to investigate another related problem: marriage for Elizabeth. Parliament in 1536 and 1543 recognized Henry VIII's right to name his successor, and by his will Henry excluded the Stuarts and favored the Suffolk line. During the turbulent 1560's the succession question affected domestic and foreign policies. The 1562 play, *Gorboduc*, a thinly veiled attack on the Stuart claim, called for Parliament to settle succession. In 1563 Parliament might have accomplished this, but no single candidate could be agreed upon.

Levine's most important chapters are devoted to a careful scrutiny of tract literature. The Suffolk claim rested upon three pillars: the common-law rule

against alien inheritance, the legitimacy of the Suffolks, and the will of Henry VIII. The Tudor publicist John Hales upheld the Suffolk claim in his *Declaration of the Succession*, and he enjoyed powerful support. Sir Nicholas Bacon was sympathetic, and Sir William Cecil was not anxious to investigate Hales even though Elizabeth suspected a conspiracy. Edmund Plowden, Sir Anthony Browne, and Bishop Leslie supported the Stuart claim and attempted to refute Hales. Levine concludes that Lady Catherine Grey's claim had more force: "The very existence of the rule against alien inheritance would seem to place the burden of proof on the Stuart side." Yet, this rule was never applied to the crown. Levine upholds the legitimacy of the Suffolks, the least contested point in the argument. The case for the Suffolks rested primarily on the will of Henry VIII, which Levine maintains was valid. "The answer to the question is that Lady Catherine Grey, not Mary Queen of Scots, had the best right to succeed Elizabeth."

By 1566 Catherine Grey had gained solid parliamentary backing. The Duke of Norfolk had been won over. Only by browbeating the Lords was Elizabeth able to control events. By 1568, with Catherine Grey's death and Mary Stuart's flight to England, the question had become purely academic. The Suffolk line was now weakly represented by Catherine Grey's and Hertford's two young boys, legally proclaimed bastards. Throughout the period 1558-1568 Elizabeth's policy "was to try to hold the line," though she favored Mary Stuart. With greater wisdom than most, she realized that no settlement of succession was the only satisfactory answer.

There is in Levine's book perhaps too much emphasis, without substantial proof, on the validity of Henry's will. Levine agrees with Pollard rather than with L. B. Smith on the matter of signature of the will versus "dry stamp signature." There is some conjecture concerning the marital problems of the principals and speculation on what might have happened if there had been a Suffolk instead of a Stuart succession. These minor criticisms aside, Levine's monograph is a solid and mature piece of scholarship.

University of Richmond

JOHN R. RILLING

JOHN PENRY AND THE MARPRELATE CONTROVERSY. By Donald J. McGinn. (New Brunswick, N. J.: Rutgers University Press. 1966. Pp. xi, 274. \$9.00.)

John Penry's major role in the publication of the Martin Marprelate pamphlets has long been recognized. Professor McGinn believes that Penry was not merely a prime mover in the Martinist conspiracy but the sole author of the tracts themselves. This study is a sequel to McGinn's several articles on the Marprelate controversy and to his able and comprehensive survey of the earlier *Admonition* controversy. As an introduction to his consideration of Penry's authorship he reviews the important tracts that bridge the period between the two controversies. Since Penry emerged as a major Puritan pamphleteer toward the end of the period, the survey, as it focuses on Penry's works, provides the logical background for an analysis of Penry's relationship to the pseudonymous tracts. McGinn believes that a careful examination of the seven tracts points to a single author. Thus

Penry, the only major Martinist involved in the production of all the tracts, emerges as the obvious candidate. McGinn's detailed re-examination of the depositions of the various persons investigated between 1588 and 1590 strongly supports this view. References in the writings of the Separatist Barrow and the ablest of the anti-Martinists, Nashe, make it clear that both believed Penry to be Martin. McGinn's most convincing argument, however, is based on internal evidence, a comparison of the style of Penry's acknowledged writings with that of the tracts, the similarity of the personal allusions throughout both sets of writings, and the evidence of typography. He himself would stress as at least equally important the evidence of Penry's ideological development, the change from orthodox Presbyterian Puritanism to Separatism. He finds similar indications of embryonic Separatism in the statements of the presumed staunch Presbyterian Martin and in passages from writings accepted as Penry's. It is an interesting thesis, but a thesis nonetheless, and hence hardly solid evidence for the primary thesis of Penry's authorship of the Martinist tracts.

McGinn's treatment of scholars who have supported the claims of candidates other than Penry is judicious, and his answers to their arguments, convincing. He has made an extremely strong case for Penry's authorship. It will stand, I suspect, since the only possibility of new evidence would be that which a computer might provide. And with Martin Marprelate, as with *The Federalist Papers*, the computer will doubtless support a case already made by sound historical and textual scholarship.

Denison University

W. M. SOUTHGATE

THE STUART CONSTITUTION, 1603-1688: DOCUMENTS AND COMMENTARY. Edited and introduced by J. P. Kenyon. (New York: Cambridge University Press. 1966. Pp. xvi, 523. Cloth \$12.50, paper \$4.95.)

THE purpose of this volume is "to provide a representative and easily accessible selection of documents concerned with, and often in themselves comprising, the political and constitutional history of the seventeenth century in England." Professor Kenyon has cast a wide net, and the 146 documents that he has compiled will fascinate and instruct a whole generation of Stuart scholars. Their variety can only be suggested by noting that he has reprinted extracts from such diverse sources as the Putney debates, the inflammable *Appeal from the Country*, and the decree of Oxford University that set the stage for the last years of Charles II. There are some noticeable omissions. No extracts are given, for example, from the well-known Apology of the House of Commons (1604), probably because of G. R. Elton's recent essay showing that it represented a minority opinion in that house. But surely more will be written on this subject. More important, the kingship was the very center of the Stuart constitution, and at least one writer has stated that the death of Charles I was the cardinal event of the seventeenth century. Space should have been found for his trial and the defense that helped fashion the legend of a monarch martyred for his people's liberty. It should also be pointed out that the documents of the revolution settlement are not in this vol-

ume but in its companion, E. N. Williams' *The Eighteenth Century Constitution*.

Kenyon has provided an able commentary based on the most recent historical scholarship, which strikes hard at the interpretation of the Stuart constitution that has arisen from S. R. Gardiner, Sir Charles Firth, J. R. Tanner, and Macaulay. He is often persuasive as in his account of the ascendancy of Coke and the reaction against him or in his comments on the Star Chamber before the Civil War. But some generalizations will be questioned. One of these is the assertion that civil war did not erupt during the Exclusion crisis because too many of what the author calls the Commons gentry "believed implicitly in the absolutism of monarchy and the sanctity of non-resistance." Though nonresistance was elevated into a creed after the restoration of Charles II, it is doubtful, nevertheless, that absolutism possessed either in this or in any other period of the seventeenth century the popularity attributed to it here. Yet *The Stuart Constitution* is undeniably an impressive work of scholarship that will command general admiration, and Kenyon's interpretation will stimulate much controversy and discussion.

Hunter College

CORINNE COMSTOCK WESTON

STORIA D'INGHILTERRA. Volume IV, L'IMPERO: DAL 1603 AL 1702.

By *Mario M. Rossi*. (Florence: G. C. Sansoni, Editore. 1966. Pp. 676. L. 6,000.)

SIGNOR Rossi has undertaken a full treatment of English history in Italian, presumably for the use of Italian students of the subject. In this volume he touches upon all aspects of Stuart England in which such students are likely to be interested. Not only are the social, economic, ecclesiastical, constitutional, and political aspects of English life considered; the author has much to say about early colonization and trade with outlying parts of the world. Of this American colonial historians should be aware. The book is in general praiseworthy for the clarity and exactness of writing and for organization (apart from inclusion of much content in footnotes). The author's views are freshly formed, based on wide reading and research, and usually sound.

On the other hand, Rossi's treatment of the Revolution of 1688 is marred by scorn for all concerned: William of Orange, James II, Whigs, Tories, and even the Seven Bishops. There is a growing wish for revision of the older histories of the Revolution, but it is unlikely that revision of this sort will be finally acceptable to serious historians.

Factual mistakes are few: Godolphin was not Secretary of State at the accession of James II, as the author tells us; James reigned not thirty-four months but about forty-six; on page 590, "Maria" is written when surely "Anna" is meant.

This book with one important flaw is still a useful one; it should be translated and made available to scholars and students of the English-speaking world as it has much to offer them. An index and a more adequate bibliography might be added to such a version.

Eastern Illinois University

GEORGE HILTON JONES

THE GROWTH OF RESPONSIBLE GOVERNMENT IN STUART ENGLAND. By *Clayton Roberts*. (New York: Cambridge University Press. 1966. Pp. xi, 466. \$13.50.)

CLAYTON Roberts does not claim that 1603 saw the first dawn or 1714 the final triumph of the principle of ministerial responsibility to Parliament, but he does maintain, with sound scholarship and considerable success, that the Stuart years saw the evolution of the several and various political concepts necessary to the ultimate establishment of such responsibility.

The opening chapters offer certain problems, for, as Roberts makes clear, early Stuart kings and Parliaments alike had no idea of ministerial responsibility to anything other than the monarch nor of any supremacy other than royal. The study must, therefore, be of policies and actions projected for particular ends within one framework of government, but leading sometimes to results quite unexpected and different from those designed, and often very indirectly to a new political framework, which, in fact, if ever suggested, would have seemed quite unintelligible to those who were actually in the process of creating it.

Roberts deals with this by centering these chapters on the impeachments of certain leading ministers of Kings James I and Charles I. Stuart subjects recalled Tudor successes both in maintaining high standards of loyalty and performance among their servants and in trimming sails of policy to prevailing political winds. The lack of such achievement by their Stuart successors led Parliament to revive the medieval process of impeachment, first to punish miscreant ministers left to their evil ways by royal inattention, and then, more seriously, ministers of good conduct whose only sins lay in too close a loyalty to the king or too active a part in the invention or execution of royal policies unpopular with Parliament. Concentrating on the impeachment conflicts, Roberts reveals clearly the complex of legal, constitutional, and political questions they raised, the implications of the newly developing idea that the king could do no wrong and its significance for ministers who might serve him, and the problem of determining which ministers advised which policies, among them.

This concentration upon impeachment is, however, less successful when the author deals with the Restoration period. For impeachment soon proved an unhandy weapon, illuminating perhaps, but hedged about with legal and political difficulties. The Cavalier Parliament did not neglect the tradition, but it did cast about for more efficient ways of forcing ministers to their will and now, occasionally, their candidates into the ministry. Roberts does take some note of the promising issue of financial pressure. The reader might willingly sacrifice details of Danby's defense for more extensive attention to the significance of that minister's efforts to establish a Parliamentary party, and to the other ways resourceful opposition politicians attempted to bend ministers to their wills.

In the closing chapters, Roberts does turn to such problems. With impeachments all but abandoned and the principle of parliamentary supremacy established, post-1688 Parliaments tried to make the ministry responsive if not yet responsible to them. Parties and political leaders, circumstances and events, old traditions and new ideas are explored in this last, and perhaps most telling part of the study,

as questions of theory give way almost entirely to questions of practical politics. The book is important because it gives a new and provocative view of Stuart political development; it is interesting because it is written with imagination, judgment, clarity, and style.

University of Illinois, Chicago Circle

CAROLYN A. EDIE

THE HISTORIOGRAPHY OF THE BRITISH EMPIRE-COMMONWEALTH: TRENDS, INTERPRETATIONS, AND RESOURCES. Edited by *Robin W. Winks*. With twenty-one essays by *George Bennett et al.* (Durham, N. C.: Duke University Press. 1966. Pp. xiv, 596. \$12.50.)

CONTRIBUTORS to this large and important book, the first in its field, maintain a high level of performance, and the book is a valuable aid to and deserves a warm welcome from historians and students of Commonwealth and international affairs. The twenty authorities, eleven drawn from the Commonwealth, survey in twenty-one "essays" the trends, as they see them, revealed by recent historical writings in this field. The result is a stimulating space age view of the expanding universe of Commonwealth studies, or, more accurately, of studies of the countries and areas, once or still parts of the Empire-Commonwealth. As the editor notes, "the national orientation of the essays reflects the traditional approach. . . ." The scope of the work is expanded sufficiently to include essays on areas like British territories in the Pacific—Malta, Cyprus, and Gibraltar; and areas formerly linked with the Empire—Egypt, the Sudan, Burma, and "The American Continental Colonies" before 1776. "The Empire since 1783" traces changes culminating, in the last decade of research, in a "revolution" that includes a breaking out from the "narrow compass of political constitutional history" and the discrediting of the label that dubbed the late Victorian Empire "The Age of Imperialism." ("In no other era of British Empire history has so much dogmatism rested on so little evidence.") Other general themes are explored in "Trusteeship and Mandates" and in "Commonwealth Literature." The emphasis of the volume, as the editor—who also contributes an important chapter on Canada—points out, is on "recent" literature and the "newer" interpretations. "Recent" means roughly 1939–1963. Fortunately, nearly all the essays take the time limits lightly; they do not hesitate to go back to early historical writings or forward to books published as late as 1965. Among "newer" phases, the editor points to one outstanding feature: "When the field first developed professionally, it was unhesitatingly London-oriented; in the 1940's and 1950's it was as strongly driven to its many corners." So strong is the drive to the "many corners" that the book contains no chapter on the United Kingdom—"intentionally omitted, of course," as the editor puts it. This omission, and the lack of any chapter focused on the Commonwealth itself, leaves a void in the center that none of the marginal references to the United Kingdom and the Commonwealth can fill. True, the national chapters throw their nets wide and capture bits of the Commonwealth, but most of it slips through the meshes and between the nets. A chapter devoted specifically to recent United Kingdom historical writings would have brought in some reference to studies important for the history of the Commonwealth, but it finds no place in

the volume. British war histories, which bear at many points on the Commonwealth, are not mentioned (though the national chapters feature their own war histories). Nor is there any reference to other important sources, such as Lord Hankey's *The Supreme Command, 1914-1918* (1961), L. S. Amery's three-volume *My Political Life* (1953-55), or J. E. S. Fawcett's historically oriented *The British Commonwealth in International Law* (1963). Such questions of coverage are important since, as the reader is told, the book is designed to "help librarians . . . to build collections . . . based on the works discussed here" and to serve as "a bibliography and guide for scholars [and] students. . . ." Their attention might have been drawn to the bibliography, one of the most extensive in existence, in *The Cambridge History of the British Empire*, Volume III, *The Empire-Commonwealth*. More clues are needed than the student is given to a number of aspects of Commonwealth relations.

Bethesda, Maryland

H. DUNCAN HALL

A HISTORY OF AGRICULTURAL SCIENCE IN GREAT BRITAIN, 1620-1954. By Sir E. John Russell. Foreword by Sir Bernard Keen. (New York: Humanities Press. 1966. Pp. 493. \$10.00.)

THE former director of the Rothamsted Experimental Station wrote this comprehensive history shortly before he died in 1965. In recounting the major events in British agricultural science, Russell chose to define agriculture simply as husbandry, thus excluding processing, marketing, and transportation almost entirely. In practice he defined science as biology and chemistry and excluded mechanics, engineering, and technology in general. Within these boundaries, however, the author synthesized a wide range of topics and information. Indeed, the variety and quantity of material make even the briefest summary impractical. The book covers nearly everything and everyone within its scope. Non-British contributions before about 1850 get a fair discussion and evaluation, but foreigners get abrupt treatment for the years after 1850. Justus von Liebig, for example, receives much attention, but Louis Pasteur, Gregor Mendel, and Robert Koch are barely mentioned. The author scarcely refers to Americans, and then chiefly when they were proven wrong. Russell misunderstands the operation of the experiment stations in the United States, but this may be related to a common European inability to comprehend federalism. Still, his history does not pretend to be universal or complete; it deals only with agricultural science in Great Britain.

The author gives some short, well-done biographical sketches of leading scientists. Russell was trained as a chemist, and he tends to favor chemistry over biology and plants over animals. Even so, he often shows interdependencies and notes how discoveries in one science have frequently depended on or led to discoveries in other sciences. He shows, for example, how ignorance of nitrate fixing bacteria virtually stalled work in certain areas of soil chemistry. The interdependencies of the sciences and the troubles in raising money for research share about equally as his main themes. The book will no doubt become a classic reference for many years.

Smithsonian Institution

JOHN T. SCHLEBECKER

THE GROWTH OF THE BRITISH PARTY SYSTEM. Volume I, 1640-1923; Volume II, 1924-1964. By *Ivor Bulmer-Thomas*. (New York: Humanities Press; distrib. by Hillary House Publishers, New York. 1966. Pp. x, 344; 328. \$16.50 the set.)

IN this lucid but conventional narrative of British party politics over the past three hundred years, the main emphasis falls upon the present century. The principal actors in the story are the party leaders at Westminster; its central theme is how they have won and lost power within the British system of government and within their respective parties. The author recounts in considerable detail such events as the constitutional crisis of 1910-1911, the overthrow of Asquith and the formation of the Lloyd George government in 1916, the breakup of the coalition in 1922, the formation of the national government in 1931, the struggle for the Labour leadership in the 1950's, and the choice of Home as Conservative leader in 1963.

In spite of its title, the book makes virtually no effort to mark out the main periods or stages in the "growth" of the parties and the party system. Little attention is paid to the development of party organization in the country or in Parliament, the distribution of power within the parties, the socioeconomic bases of political behavior, or the role of ideas in shaping party policies. Although the specialist in British history or government will find little that is new in substance, interpretation, or approach, the book is based upon wide reading in standard biographical and secondary works and will be a useful supplement to more analytical studies.

Harvard University

SAMUEL H. BEER

A HISTORY OF THE LONDON GAZETTE, 1665-1965. By *P. M. Handover*. (London: H. M. Stationery Office; distrib. by British Information Services, New York. 1965. Pp. vii, 95. \$3.50.)

THIS brief, official history, written to celebrate the tercentenary of the oldest English newspaper, traces its changing role as servant of crown and executive from a commanding position in news presentation and public influence in the late eighteenth century to its present specialized function as purveyor of official announcements and legal notices. Profiting by access to diplomatic sources, the early *Gazette* fortified the prerogative by featuring accurate reports of foreign and trade developments, as well as advertisements of particular interest to the governing classes. Its quasi monopoly, reinforced in the beginning years by direct and indirect methods, was broken by the Commons' refusal in 1694 to renew the Licensing Act. Within a decade a number of rival sheets, including the daily *Courant*, had rushed into print, outbidding the *Gazette* for the favor of both officialdom and "generality" through livelier, if less responsible, journalism. The paper began a long retreat into dignified (sometimes stuffy) impartiality and limited usefulness.

Along this line of march many landmarks of English constitutional history can be seen in fresh perspective for the *London Gazette* served throughout the

period when the court created ministries into an age when its columns mainly reflect the sovereign will of the cabinet and permanent civil service. Miss Handover's treatise is full of interesting detail on its relationship to various ministers, reforming and otherwise, its frequently amateur editorship, its varying fiscal fortunes, and its changing make-up. Her work reflects admirable research and her intimate knowledge of the printing trade. This essential volume, enhanced by the illustrations, suffers from a lack of index and bibliography, and it is, alas, sometimes couched in a style that makes a fascinating subject appear needlessly obscure.

University of South Carolina

GEORGE CURRY

CHICHESTER TOWERS. By *L. P. Curtis*. (New Haven, Conn.: Yale University Press. 1966. Pp. xvi, 114. \$5.00.)

TAKING off from Namier's debatable pronouncement that, since historical comedy would have to recognize the deeper irrelevancies and incoherence of human actions, it is never written, Professor Curtis has sought to repair the gap. Whether readers will find him successful depends on their patience. His effort stands on the seemingly disparate conjunction of a vacant deanery and a parliamentary election. But as even—one is tempted to prefer "only"—the most superficial scanner of eighteenth-century English history knows (omitting Macaulay's omniscient schoolboy), the disparateness is but seeming. Religion and politics had much in common with genius and madness. How, indeed, could it be otherwise in an Erastian world? How could it be otherwise in any world where the Church played a part later played by more obviously secular actors?

What the author has done on his small stage—Sussex, home of the Pelhams—is to present a large cast, some but marionettes, some most considerable persons. The Duke of Newcastle is there, the Duke of Richmond, too; offstage but still an influence is Edmund Gibson. Onstage are lesser bishops, deans, canons, nobles, gentry, and, most central of all, Archdeacon Thomas Ball, the man who would or would not be dean. The comedy is Balzacian not Gilbertian, though the author of *Iolanthe* could have found abundant material for his comic muse—spongers, pretenders, little gentlemen in black velvet gowns who burrowed under the churchyard. Nor would he have missed the one welcome exception in this tedious cast, William Clarke, canon residentiary of Chichester Cathedral. A man of no ambition, Clarke loved bathing in the sea, buying fish, viewing old Saxon camps, and counting ships. Who could resist his bidding to come to Brighton where "we do just what we like" and "are bound by no conventionalities"? He has his value as well as his charm, for he is proof tangible that not all Georgian clergy dwelt on place and interest; some had learned that these things were not worth their notice. Had we had more of him and less of a dozen others, we should have learned quite as much about human irrelevancies and incoherence.

University of Missouri

CHARLES F. MULLETT

THE MATHEMATICAL PRACTITIONERS OF HANOVERIAN ENGLAND, 1714-1840. By E. G. R. Taylor. (New York: Cambridge University Press for the Institute of Navigation. 1966. Pp. xv, 502. \$16.50.)

THIS erudite and expensive book is a sequel to the author's *The Mathematical Practitioners of Tudor and Stuart England* (1954). The work is made up of two main parts: "Narrative" and "Biographies." The first contains an account, subdivided into eight chapters covering about two decades each, of the activities of the more significant "mathematical practitioners." The emphasis throughout is placed on the noun rather than the adjective, for the author describes in detail the technical and entrepreneurial problems of her practitioners without coming to grips with the relationships between pure and applied mathematics. One of the chapters is called "The Great Instrument-Makers"; this phrase might appropriately have served as a title for the book. Most of the volume is made up of biographies, chronologically arranged generally by decades, of some 2,200 practitioners, most of whom were makers of instruments. About five times as many biographies are listed here as appeared in the earlier volume on Tudor and Stuart England, and hence many of the items are brief indeed.

Professor Taylor's earlier book included a third part on "Works on the Mathematical Arts and Practices," but in the present volume citations of such books form part of the biographies of Part II. These biographies now helpfully provide indications of the sources of the information (through a system of abbreviations). The volume is graced by a dozen attractive plates.

In her books of 1954 and 1966 the author has given us a reference set that contains a wealth of detailed and useful information on a period of three and a half centuries in the development of science. The *terminus a quo* of the second volume is perforce the year of the death of Queen Anne and the accession of George I, but for the *terminus ad quem* no date of comparable precision was available. The closing year, 1840, does not mark an event; it corresponds only to a trend. "By the year of Queen Victoria's wedding it had become virtually impossible to pick out the mathematical practitioners as a distinct professional group. . . . The scientist had now disappeared into the laboratory, the craftsman into the factory, the private teacher had become a schoolmaster or a professor, the best surveyors were in the army or the navy. All had parted company, and the work of a majority was anonymous."

Brooklyn College

CARL B. BOYER

THE CORRESPONDENCE OF JAMES BOSWELL AND JOHN JOHNSTON OF GRANGE. Edited by Ralph S. Walker. [Boswell's Correspondence, Volume I.] (New York: McGraw-Hill Book Company. 1966. Pp. 1, 369. \$17.50.)

THIS is the first of some forty volumes projected for the "Research Edition" of Boswell's archives now at Yale. The series will obviously be a "standard work," and hard-pressed librarians, the Canutes in this age of editors, must begin to look to their brooms. The earlier "Reading Edition," already running to nine volumes,

presents selections from the Boswell Papers principally for the benefit of the general reading public. The "Research Edition" aims at an authoritative, exhaustive publication of the entire corpus for scholars and specialists. Eventually, the new series will consist of three groups of volumes: the journals, diaries, and memorandums; the correspondence; and the *Life*. At \$17.50 per volume, collectors will find the capital outlay considerable.

The correspondence of James Boswell and John Johnston spans almost three decades, from the former's student days at Edinburgh University to the death of the latter in 1786. Of a total of 164 letters, by far the greater portion from Boswell, of course, 142 have survived. Their publication is a superlative achievement of editorial scholarship. Texts are faithfully reproduced; headings are painstakingly helpful and informative; and footnotes are rich to the point of rococo.

What Boswell and his older friend wrote to each other impinged scarcely at all upon the great world. The value of their correspondence lies rather in the information it gives on Boswell's private life, particularly on his truly charming capacity for friendship with a fellow Scot whose temperament differed radically from his own. Johnston was modest, awkward, indolent, provincial, the perfect foil for the ebullient, egocentric, ambitious young man in love with the *haut monde* and passionately longing to make it his native habitat. There were also important common denominators, however: background, the law, tavern haunting, wenching, the "spleen." More important, truer gauges of character were the decent, unswerving, manly loyalty and the mutual support in times of need.

Boswell was the gainer. Much the more complex, he was granted the opportunity to be himself. (Even so, he exhibited his blend of rationalism and romanticism: "I am a Sceptic, But a devout one.") He used Johnston, admitting with saving ingenuousness, "I cannot help reflecting on myself," a harping upon a string "which one would think must e're now have been thrumed to pieces." For Johnston, the amiable and willing confederate, the reward was a sidelong glance from posterity.

Southern Methodist University

CHARLES R. RITCHESON

BRITISH SUBMINISTERS AND COLONIAL AMERICA, 1763-1783. By Franklin B. Wickwire. (Princeton, N. J.: Princeton University Press. 1966. Pp. xii, 228. \$6.00.)

THIS brief study "attempts to analyze the importance of secretaries and undersecretaries in five important British administrative departments to the determination of British colonial policy during the period of the American Revolution."

The first half discusses the departmental system in which these officials served their superiors. In so far as this is concerned with organization and procedure, it is a useful, and indeed necessary, introduction (although Miss Jucker's summary of the Undersecretary's duties on page thirty-four should not have been quoted as the actual words of a contemporary). The more general aspects of the system, with its emphasis on patronage, are fairly well known, but they are full of subtleties, and the author seems at times to take conventional politenesses too seriously and to make categories out of infinite gradations. The specific activities of

"subministers" demand extended evidence to illustrate adequately the range and degree of their undoubted influence, and to do more than add detail to what is already familiar in outline. In some hundred pages the author presents carefully chosen samples of this activity by showing how the chief measures of American policy were arrived at from 1763 to the outbreak of war and, more slightly, during the war itself. If there is any lingering belief that these measures sprang fully matured from the minds of Grenville, Townshend, and other responsible ministers, this study should dispel it. The careers of Thomas Whately, John Pownall, William Knox, Philip Stephens, and John Robinson afford good examples of their participation and initiative; a few others appear incidentally. Comparatively little use is made of Charles Jenkinson or William Eden, who do not stay in the bureaucratic niche, but become classic cases of graduation into the aristocracy.

The author relies heavily on monographs and articles by historians who have examined parts of the subject in depth, but he adds much information from archival sources. The bibliography is too diffuse for its immediate purpose.

University of California, Berkeley

G. H. GUTTRIDGE

A HISTORY OF INCOME TAX. By *B. E. V. Sabine*. (New York: Humanities Press. 1966. Pp. 288. \$6.75.)

THIS history of the *British* income tax has several virtues: it is the first account spanning the whole course since its wartime beginning in 1799 under the name "property" tax; it is based on extensive use of the monographic and pamphlet literature, the parliamentary debates and reports, and the administrative records; and it is written with clarity and precision—with very few slips of pen or type—as a labor of love by an insider, an Internal Revenue officer, sensitive to policies and to administrative developments. What faults it has do not seriously detract; they flow, perhaps in some part, from its virtues. The definition is sometimes a bit insular, assuming a ready familiarity with such terms and issues as the coverage of Schedules A through E that non-British readers may not possess. A sort of subtitle on the jacket promises that the development of the income tax is "related to the social, economic, and political history of the period," but references to social and economic change, or to the relative role of the income tax among other sources of revenue, are not very systematic or sufficient. A few period tables, or an appendix, with consistent series and with the better estimates of GNP and per capita income (see Mitchell and Dean, *Abstract of British Historical Statistics* [1962], not listed in the bibliography) would be useful toward supplying perspective.

There are an excellent index and a useful bibliography although the latter is by no means complete even for works cited in the text, sometimes with that unfortunate *op. cit.* Incidentally, Sir Henry Parnell's classic *On Financial Reform*, which is cited in the bibliography, but is not mentioned in the text, was first published in February 1830, not 1832. It was read by Peel, may well have influenced his proposal in the cabinet in March 1830 for an income or property tax devised, as Lord Ellenborough wrote in his *Diary*, "to reconcile the lower with the higher classes and diminish the burthen of taxation on the poor man," and in other re-

spects foreshadowed his reforms beginning in 1842. This is a useful work that need not discourage the writing of a better one and cannot be ignored when a better one comes to be written, perhaps by Mr. Sabine, himself.

Tufts University

ALBERT H. IMLAH

JOHN BRIGHT: VICTORIAN REFORMER. By *Herman Ausubel*. (New York: John Wiley and Sons. 1966. Pp. xvi, 250. Cloth \$5.95, paper \$2.95.)

SINCE the appearance of G. M. Trevelyan's classic life of Bright more than a half century ago, historians have left the tribune of the northern manufacturing middle (and in some degree the working) classes relatively undisturbed. Except for a few specialized monographs and Asa Briggs's admirable essay in *Victorian People*, the output has been of little consequence, and it has not seriously modified Trevelyan's picture. In making a fresh study of the great Lancashireman, Professor Ausubel has canvassed a wide range of manuscript sources in British and American archives, and he is able to correct certain, though not, I think, particularly vital, errors of interpretation. His book is relatively brief, agreeably written, and attractively illustrated, chiefly with cartoons from *Punch*.

What Ausubel's researches seem to show is a Bright much less self-confident, more given to doubts and uncertainty, than we have thought. Behind the eloquent thunderings of the great orator—indubitably he belongs among the three or four greatest orators of his century—stood a man often desperately dependent on others. During the 1840's George Wilson and Richard Cobden gave him support; after his remarriage in 1847 his wife and his growing family provided something of a sense of security. But Bright's two breakdowns, one growing out of his exertions against the Crimean War and the other in 1870 when a series of bitter fights was looming up before the Gladstone government in which he had taken office, are suggestive psychologically, and I wish that Ausubel had been able to analyze them in greater detail. Clearly there was a crack in the façade of solid self-confidence that Bright usually exhibited to the world. Far from never having had any doubts, at certain stages in his career, on Ausubel's evidence, he was wracked by them.

Bright is revealed as the champion of free trade, democracy, and peace, and the opponent of squires, parsons, and generals, and his love-hate relationship with the middle classes is duly noted. Still, in some measure, the book leaves one with the uneasy feeling of important questions unresolved and, indeed, unasked. For example, what influence did Bright's Quakerism and his Lancashire rearing have upon his social outlook, his sense of grievance against the aristocracy and the Church, and his passion for stating issues of public policy in terms of morality? In reality, how radical, or conservative, was Bright's prescription for mid-century Britain? What was his attitude, *au fond*, to state intervention? No doubt everyone to the Left of Herbert Spencer had some areas in which he would permit, even encourage, the state to intervene, and Bright, as a Nonconformist, was clear that to provide elementary education was the duty of the state. But when we come to such enterprises as factory legislation, public health acts, and the like, he emerges as an uncompromising and unrepentant individualist. Here Ausubel's assurance

that Bright "had never been a doctrinaire advocate of laissez faire" seems to call for a more detailed explication. This, perhaps, is only to regret that his book is not longer.

Harvard University

DAVID OWEN

ESTATE VILLAGES: A STUDY OF THE BERKSHIRE VILLAGES OF ARDINGTON AND LOCKINGE. By *M. A. Havinden* with contributions by *D. S. Thornton* and *P. D. Wood*. [Museum of English Rural Life.] ([London:] Lund Humphries for the University of Reading. 1966. Pp. 210. 50s.)

ARDINGTON and Lockinge were (and are still) the chief villages of a noted Berkshire landed estate. Acquired piecemeal by the bankers Lewis Loyd and his son Lord Overstone in the mid-nineteenth century, the Lockinge estate became the property of the latter's daughter and her husband, Colonel Robert Lindsay, a winner of the Victorian Cross in the Crimean War and later known as Lord Wantage, under whose guidance during the years of agricultural depression a large part of the estate was taken in hand, farmed directly by the owner, and made the scene of an interesting agricultural experiment. Lord Wantage died in 1901, but Lady Wantage continued his work until her own death in 1920. A cousin, A. T. Loyd, an official in the Egyptian civil service, succeeded them and farmed about ten thousand acres himself until his death in 1944. Death duties then forced the sale of about one-quarter of the estate (five thousand acres), but this still left a large property partly farmed by the late owner's son, C. T. Loyd, and partly leased to tenants.

Mr. Havinden and his associates decided to make a detailed study of life on the Lockinge estate from its beginning until now, concentrating chiefly on such matters as landownership, estate management, the economics of farming, and the social life of the inhabitants in the villages of Ardington and Lockinge. The scope of the work is thus wider than its title would suggest. About one-third of the book is devoted to the Lockinge estate's history since 1945, and for this Havinden has added the skills of the rural sociologist to those of the social and economic historian. The early history of the estate, prior to and during its acquisition, has been carefully pieced together by painstaking study of estate deeds, tithe surveys, and enclosure awards, and some of the findings will be of interest to economic historians—particularly the improved cultivation of unenclosed land and the high prices paid by rich bankers for their estates. Although Overstone's correspondence with his fellow banker, G. W. Norman, has been unearthed and used, it disappointingly contributes little on the personality and motives of one of the greatest estate builders among Victorian bankers. More disappointing still, no use at all is made of estate accounts, an omission that tends to lessen the value of the book's discussion of estate management. The detailed analysis of village life in the mid-twentieth century is most useful, not least because it makes one wonder about the beneficence of Wantage's rural experiments a half century earlier.

Johns Hopkins University

DAVID SPRING

FROM THE DREADNOUGHT TO SCAPA FLOW: THE ROYAL NAVY IN THE FISHER ERA, 1904-1919. Volume III, JUTLAND AND AFTER (MAY 1916-DECEMBER 1916). By *Arthur J. Marder*. (New York: Oxford University Press. 1966. Pp. xxiii, 306. \$11.20.)

THIS third volume of Marder's magnificent history of the Royal Navy between the appointment of Fisher as First Sea Lord and the scuttling of the High-Seas Fleet brings the story to its climax. The Battle of Jutland itself marked a turning point that was accentuated by the abortive German sortie of August 19, 1916. At Jutland for the first and only time the full British and German battle fleets actually engaged. But neither admiral wanted a knockdown fight; each, in fact, had to preserve his fleet. The result was an indecisive encounter that the Germans, by swift use of the press, claimed as a victory. The British Admiralty bungled the public relations side of the conflict and left the impression that the Royal Navy had let Britain down. True, some things had been found wanting, but this was, as Marder correctly points out, bound to be the case when so much matériel had never been tried in battle. Both these defects and those in the overcentralized command structure were rapidly rectified, and Marder reckons that by August the Royal Navy was twice as good as it had been and would have, given the chance, soundly trounced the High-Seas Fleet.

The period covered in this volume is from just before Jutland to the end of 1916, when Balfour and Jackson were forced from the Admiralty and Jellicoe fatefully accepted the post of First Sea Lord. This is the time in which the situation changed from a struggle between the great dreadnoughts commanded by senior officers to the threshold of the battle between submarines and escorts fought by junior officers. The story of this latter conflict and of the evolution of the naval air arm will be contained in the fourth and final volume.

For the historian, the year 1916 at sea is no easy task to handle. The historiographical problems are vast, ranging as they do from the intricacies of the Grand Fleet Battle Orders to the interminable wrangling between the Jellicocites and the Beattyites. With calm skill Marder picks his way with meticulous accuracy and unbiased judgment through the morass of technical and conflicting claims, setting aside emotional assessments and service politics to come to conclusions that will, because of the access he has had to everything known to exist on the subject, stand the tests of time. As an example of seeking, exposing, marshaling, and weighing the evidence combined with clear, readable prose, this is a volume to be recommended certainly to all graduate students and in many cases also to their tutors.

Kansas State University

ROBIN HIGHAM

A DECADE OF THE COMMONWEALTH, 1955-1964. Edited by *W. B. Hamilton et al.* [Duke University Commonwealth-Studies Center, Publication Number 25.] (Durham, N. C.: Duke University Press for the Center. 1966. Pp. xx, 567. \$12.50.)

THIS useful book, the product of a conference representing seven Commonwealth nations and the United States, was sponsored by the University of London's

Institute of Commonwealth Studies and the Duke University Commonwealth-Studies Center in Italy during mid-1964. The papers presented at the conference were published as chapters under the following six parts or sections: "The Commonwealth: A Retrospective Survey, 1955-64," "The Birth of States," "Inter-Commonwealth Relations," "Samplings of the Interchange of Institutions and Culture," "International Relations," and "Economic Relations."

After a survey of the state of the Commonwealth from 1955 to 1964, Professor Nicholas Mansergh questions whether Britain would in the later twentieth century break away from the Commonwealth as it had broken away from its empire in the mid-nineteenth century. The answer to the query can be found in such perceptive essays as W. B. Hamilton's analysis of "The Transfer of Power . . .," W. H. Morris-Jones's review of the "Political Institutions in the New Commonwealth States," Kenneth Robinson's review of the machinery of Commonwealth consultation and cooperation, Alistair Buchan's examination of "Commonwealth Military Relations," J. J. Spengler's discussion of the diffusion of economic ideas within the Commonwealth, and Dennis Austin's study of regional associations and the Commonwealth. Although sometimes heavy reading and overpowering in content, the essays in Part VI on such issues as the composition, growth, and migration of Commonwealth population, the intra-Commonwealth flow of capital and skills, the creation of capital and skills within certain Commonwealth nations, and the economic dimensions of the Commonwealth are replete with valuable insights and information.

As the editors point out, the essays high-light such negative factors as: the diversity which since 1947 has become a "motif" of the Commonwealth; the distressing absence of machinery for the settling of disputes between members; the failure of private capital to flow where the need is greatest in the Commonwealth; and the "uncomfortable and oppressive" growing gap between the have and have-not Commonwealth nations. Yet, in spite of the negative factors working against the survival of the Commonwealth, the editors believe that there is cause for optimism. Certainly, the "Abrupt and complete transfer of power, with no strings whatsoever, has not destroyed the Commonwealth." The English language, moreover, still remains "a common cement . . . in most of the Commonwealth" and offers its members "the best vehicle for science, development, and government." Indeed, it is well to avoid a "Hasty interpretation of what we see" in the Commonwealth today lest it obscure the very real possibilities for the future development of the Commonwealth and the evidence that in some respects diversity might facilitate Commonwealth unity.

An excellent format, a serviceable index, and some very informative tables dealing with economic statistics add much to the value of the work. Would that the price of the book were not so prohibitive!

Georgia State College, Atlanta

JOSEPH O. BAYLEN

OLD IRELAND. Edited by *Robert McNally, S. J.* (New York: Fordham University Press. 1965. Pp. xi, 252. \$5.50.)

DIVERSE hands make for diverse scholarship, but the eight authors of these nine

essays have accomplished their purpose: to write "an introduction to different aspects of the Christian civilization of Old Ireland." Although a majority of these studies represent restatements of earlier work and, in the case of Ludwig Bieler's nonetheless worth-while survey of the sources of St. Patrick's life and chronology, a position which its author has already in part abandoned, these authorities can always be read with profit by students on many levels. Besides Bieler's "The Chronology of St. Patrick," the essays of perhaps greatest interest to historians are Gareth Dunleavy's excellent recasting of his sound and sprightly *Colum's Other Island* in "Old Ireland, Scotland and Northumbria," concerning Irish relations with northern Britain in the seventh-eighth century, and Jocelyn Hillgarth's "Old Ireland and Visigothic Spain," on the archaeological and literary evidence for Spain "as principal intermediary between the East and Ireland," with Galicia as the geographic key, and Ireland's role in the transmission of Spanish authors. James Carney contributes two studies, "Old Ireland and Her Poetry," which analyzes Irish and universal elements in various poems, especially by Mael Ísa and Blathmac, and an amiable account of the background and career of the ninth-century poet-scholar Sedulius Scottus, contrasting him and other "European" elements with the "ecclesiastical Sinn Féinism" of the Culdees. The editor, Robert McNally, redeems his "Introduction," with its talk of "the European soul" and "the inner spirit of the Western man," with a sound survey of "Old Ireland, Her Scribes and Scholars," on Irish books and manuscripts, schools and education. In one of the most valuable essays, "Old Ireland and Her Liturgy," John Hennig stresses the Irish "national" sense of hagiology in liturgical and paraliturgical texts. Diarmuid O Laoghaire discusses types and expressions of early Irish spirituality, and Jeremiah O'Sullivan examines "Old Ireland and Her Monasticism," its characteristics, government, and so forth.

Closer editorial supervision could have avoided frequent repetitions of material (for example, St. Patrick's career, Irish missionary drive, monastic learning) and occasional contradictions (as, for example, on page 93, Patrick "was ordained deacon by St Amator," while, on page 25, "There is no evidence whatsoever of St Patrick's connection with St Amator.") The index is incomplete.

Lawrence University

WILLIAM A. CHANEY

DANIEL O'CONNELL AND THE REPEAL YEAR. By *Lawrence J. McCaffrey*. ([Lexington:] University of Kentucky Press. 1966. Pp. xv, 252. \$6.50.)

THIS study adds another volume to the rapidly growing collection of scholarly works on nineteenth-century Ireland. Professor McCaffrey has chosen to examine the year 1843 when O'Connell made his last great effort, with an agitation outside of Parliament, to force the repeal of the union.

The portrait of O'Connell given here is a sympathetic one, but McCaffrey's judgment is that O'Connell became a captive of past successes and failed to perceive that the situation in Britain in 1843 in no way resembled that in 1829. English opinion was solidly set against repeal. McCaffrey's narrative of 1843 is vividly told and is drawn for the most part from a wide reading of the newspaper

press and from manuscript sources. Particularly interesting is his third chapter, "Unionist Reactions." The interplay of opinion and motive among Anglo-Irish conservatives and the British government in Dublin and in London is thoroughly set out. Although numerous works have discussed this period in Irish history, in none is there such a careful working out of the day-to-day pressures and events that acted on the government of Sir Robert Peel. This chapter and the one that follows, "Peel Reconsiders," suggest the importance of studying in fuller detail and for all periods the British side of the Irish Question and examining English assumptions, prejudices, and points of view. A notable recent example of this approach is Lewis Curtis, Jr., *Coercion and Conciliation in Ireland, 1880-1892* (1963).

McCaffrey brings us very close, in his compact short study, to Peel and Graham, to O'Connell, Eliot, and De Grey, and to all the nuances of decision. What does a government do with an agitation that cannot be ignored? What does an agitator do when the promised results are not forthcoming? In this instance, O'Connell's agitation was stopped by Peel's proclamation against the great outdoor meeting at Clontarf. But Peel's creative Irish policy followed. There is much for the political scientist, as well as the historian, to study here. The major manuscript sources are listed in a bibliographical note, but newspapers and secondary sources appear in footnotes.

Connecticut College

HELEN F. MULVEY

THE IRISH STRUGGLE, 1916-1926. By F. X. Martin *et al.* Edited by Desmond Williams. (Toronto: University of Toronto Press. 1966. Pp. vii, 193. \$5.00.)

FIFTY years ago last Easter Monday a small group of extremists raised the standard of revolt in Dublin in the name of the Irish Republic. Most of the leaders realized that the revolution would fail, but they believed a "blood sacrifice" would inspire the youth of Ireland and leave them with a mission to destroy British political, cultural, and economic influence there. At first the revolution was condemned by a large majority of Irish nationalists, but the ruthless executions and imprisonment of Republicans by British authorities changed the mood of Irish nationalist opinion. The "blood sacrifice" created an enthusiastic Republican movement, a prelude to the Anglo-Irish war and the civil war that followed the treaty with Britain. Ireland's revolutionary effort and its difficulties in establishing a stable national government inspired independence movements in other underdeveloped countries and previewed some of the crises of our time.

The fiftieth anniversary of Easter Monday has encouraged a number of important publications in Irish history, including this volume of essays dealing with the period 1916-1926. Originally presented as part of the Thomas Davis lecture series on Radio Eireann, the essays discuss a number of topics related to Irish nationalism, British policy in Ireland, the sources of the twentieth-century Republican movement, the Anglo-Irish war, the civil war, Anglo-Irish relations after 1922, the Ulster question, and the establishment of the Free State. The authors write with style and objectivity, the editor has organized the essays to ensure that

they supplement and complement each other, and the material is interesting and contributes to an understanding of the forces that created the Irish revolution and the Free State. In their effort to explain and clarify rather than to eulogize, the essayists are aware of the dichotomy between the dreams of revolutionaries and the realities of postrevolutionary life and politics. Independence was far from a panacea for the ills of a backward agrarian country with impoverished urban centers, and romantic nationalist ideology provided no answers to pressing social and economic questions. But Irishmen showed more competence in conducting a war and establishing a nation than anti-Irish British opinion predicted.

Although all the essays are respectable and many are strong, a few stand out in quality: Maureen Wall's discussion of the partition issue; C. L. Mowat's evaluation of the impact of the Irish Question on British politics, 1916-1922; and Desmond Williams' observations on the difficulties and successes of the politicians in the early years of the Free State. Mowat closes his essay with this comment: "the disappearance of the Irish Question made British public life a little poorer, a little more parochial." One could add that when the establishment that controlled the Free State and later the republic rejected the artistic, liberal, and individualistic aspects of the Irish national tradition and for years tried to isolate their country from the mainstream of Western civilization they turned "romantic" Ireland into a dull, provincial, puritan little country under the influence of parish priest and shopkeeper.

Marquette University

LAWRENCE J. McCaffrey

LES PAYSANS DE LANGUEDOC. Volume I; Volume II, ANNEXES, SOURCES, GRAPHIQUES. By *Emmanuel Le Roy Ladurie*. [Bibliothèque générale de l'École Pratique des Hautes Études, VI^e Section.] (Paris: S.E.V.P.E.N. 1966. Pp. 745; 749-1035.)

SURELY this two-volume work on the peasants of Languedoc from 1450 to 1720 does not require the invocation of Georges Lefebvre's pioneering study in French rural sociology. In the last decade an entire school of French scholars led by Pierre Goubert, Jean Meuvret, Pierre Chaunu, René Bachrel, Pierre Vilar, and the late Pierre de Saint-Jacob has been exploring fiscal records, price series, notarial registers, and private archives to enlarge our understanding of French rural society in the old regime. Now Emmanuel Le Roy Ladurie mobilizes an arsenal of newly refined disciplines unavailable to Lefebvre forty years ago. Geographer, agronomist, demographer, price historian, anthropologist, and social psychologist, Le Roy Ladurie boldly assumes the risks of "total history." Most important, he does not permit his quantitative tools to obscure flesh and blood. "I began my research by adding hectares. . . ; I finished by observing the actions, struggles, and thoughts of live men."

The study is divided into five substantial sections. The first section deals with the climate, crops, rural technology, and interregional migration of a province extending from Toulouse to Lyons. The following four sections treat the author's central problem: the "great agrarian cycle" from 1450 to 1720. After a painfully

slow recovery from the disastrous depopulation following the Black Death, sixteenth-century Languedoc finally entered a period of expansion. Rising population and grain prices led to new clearings and such products as olive oil and silkworms and provided the larger tenant farmers (*marchands-fermiers*) with an increasing margin of profit. Yet these potential "rural capitalists" were unable to alter agrarian technology and substantially improve farm yields. The result was an increase in rural misery as real wages declined and the land was fragmented by ever greater numbers and by the inexorable laws of peasant inheritance. In the seventeenth century population pressures abated, and prices rose more slowly while the large proprietors raised rents sharply, causing *fermier* profits to evaporate. By 1680 prices had collapsed, and production per acre actually fell back to yields realized in the reign of Francis I. To make matters worse, all peasant proprietors were now confronted with more rigorous collection of the Church *dîme* and higher royal taxes. The sequence of rural usury, village indebtedness, foreclosure, and gradual proletarianization of both small (under ten hectares) and middling (ten to one hundred hectares) proprietors finally ended in the "adjustment" of depopulation. Malthus had won.

The crux of the problem was not monetary famine or even "micro-proprietorship" but technological immobility. And behind this were the outlook and values of the nation's elites. According to Le Roy Ladurie, aristocratic and Catholic ideals, as pursued in sixteenth- and seventeenth-century France, do not stimulate entrepreneurship and economic growth.

Johns Hopkins University

ROBERT FORSTER

LOUIS XIV'S VIEW OF THE PAPACY (1661-1667). By *Paul Sonnino*. [University of California Publications in History, Volume LXXIX.] (Berkeley and Los Angeles: University of California Press. 1966. Pp. 95. \$3.00.)

IN the author's words, this study "attempts to determine Louis' view of the Papacy from 1661 to 1667 as a contribution to the King's biography." The technique is an excellent one, and it is used with authority and skill by Sonnino. First, he isolates a subject that held Louis XIV's interest (Franco-papal relations) and limits it to a manageable and meaningful period (Alexander VII's pontificate, the celebrated Créquy incident). Secondly, and more significantly, the author seeks out the Sun King by piecing together letters the monarch actually wrote or closely supervised, by comparing the various editions of Louis's "memoirs," and by drawing on the relevant ministerial and ambassadorial correspondence, instructions, and briefs. I am convinced that Sonnino succeeds in discovering the real Louis XIV. It is only a pity that the author does not give more indications of the enormous detective work involved in deciding what specific letters are stamped with the brand of the Grand Monarch. What we read in the ten-page introduction and scattered throughout the rest of the monograph is exciting but rather general. And one must refer to the author's excellent article on the dating and authorship of Louis's "memoirs" to supplement what he says here on that subject.

Sonnino's hypothesis is that the King had little but disdain for the papacy, and that this attitude stemmed largely from the very independence of the Holy See.

The details of Louis's "views" of papal corruption and weakness are less important than the author's tangential contribution to our understanding of the personality and métier of the Grand Monarch. Louis's skillful handling of diplomacy, his independence of advisers, his dogged insistence on the international prestige of France, and the curious combination of a born optimist and flexible tactician with a frail man enslaved by fixed ideas and emotional reactions to specifics are all there for the reader to discover, but the author does not let them occupy the center stage of his drama. We await a major work on Louis XIV, the outlines of which are visible in this brief, and tantalizing, work.

University of Southern California

A. LLOYD MOOTE

GILBERT ROMME (1750-1795) ET SON TEMPS: ACTES DU COLLOQUE TENU À RIOM ET CLERMONT LES 10 ET 11 JUIN 1965. [Faculté des Lettres et Sciences humaines de l'Université de Clermont-Ferrand. Publications de l'Institut d'Études du Massif Central, Number 1.] (Paris: Presses Universitaires de France. 1966. Pp. 222. 15 fr.)

THIS colloquium can be regarded as the first fruits of the professorship at Clermont-Ferrand of Albert Soboul, its initiator. It was intended to bring together the complementary purposes and contributions of general historical scholarship and local erudition. The subject was well chosen. Romme was a student at the Oratorians' *collège* in Riom, a Freemason, the tutor of young Stroganov, before becoming an important figure in politics in Auvergne, a Jacobin, and, in *Prairial* of the Year III, one of the handful of members of the Convention who chose suicide as a means to turn political defeat into a kind of moral victory. All these indicated themes receive attention from one or more of the fifteen contributors. Useful discoveries in Italy and Russia are reported here. Galante-Garrone edits the minutes of the meetings of Romme's political club, the *Amis de la Loi*, for January 1790, which he found in the Biblioteca Apostolica Vaticana after completing his authoritative biography; and Dalin presents new information on the enforced return of Stroganov to Russia.

Two contributions seem to me particularly suggestive: the account of Romme's family, by Bouscayrol, and the analysis of the significance of his suicide, by Andrews. Marshaling details from parish registers and property records, Bouscayrol shows that Romme's grandfathers and father, dividing their energies between bourgeois professions and landed property, had been pursuing traditional family ambitions with prudence and gradual success. (An instructive comparison is presented in the equally solid account, by Leymarie, of the origins of another politician from Auvergne: Carrier.) Romme was respectable, and also sensitive. That he thought of killing himself, and that he actually did so, evidently requires explanation. One-third of this volume is devoted to suicide, as viewed by Montesquieu and Rousseau, as exemplified by Rühl two weeks in advance of Romme and his five companions, and, in the contribution by Andrews, as an act resulting from Neo-Stoic conceptions to be found in Cicero and Rousseau. The effect of Andrews' remarkable discussion is to make clear the revolutionary character of a particular sort of republican ideal of the virtuous legislator. Romme's social

origins and his martyrdom are seen in another perspective in Soboul's opening presentation and Dautry's conclusion. They view Romme as an embodiment of contradictions inherent in the society, and his suicide as a way of escaping from those contradictions. Their perspective seems to me somewhat systematic, but by insisting on the wider context of Romme's life they add considerably to the value of this stimulating collection.

Stanford University

PHILIP DAWSON

ROUSSEAU AND THE FRENCH REVOLUTION, 1762-1791. By *Joan McDonald*. [University of London Historical Studies, Number 17.] (London: University of London, the Athlone Press; distrib. by Oxford University Press, New York. 1965. Pp. x, 190. \$5.60.)

PUBLICATION of this doctoral thesis of Mrs. McDonald (nee Bedale), written a decade ago at the University of London, is welcome for it is a valuable contribution to the history of ideas during the French Revolution. As she makes clear in her second chapter, much that has been written about the relation of Rousseau to the Revolution has failed to get at the historical reality, if for no other reason simply because so many writers have been content to deal in generalities. Having restricted her study to what is essentially the period 1788-1791, she has been able to probe much deeper than any of her predecessors in the field. In the chapter on "Rousseau and the Revolutionary State" she penetrates further than others, including her mentor Professor Cobban, have had occasion to do, into the political theory of Rousseau as viewed in the context of the actual political concerns of the period. Particularly good is her exposition of the political setting in which the ideas of Rousseau were discussed and had their slight influence. The role of Rousseau in the French Revolution was slight indeed, and contradictory as well, as others have pointed out. She expresses well the general conclusion of students in the field when she says: "While the revolutionaries were prepared to use Rousseau's name when it was convenient, and to appeal to his authority in general terms, they clearly did not regard his theories as having any immediate or direct practical value in the development of their new political institutions. The power of 'doctrine,' 'abstract reason' and 'ideologists' over the minds of the revolutionaries has perhaps been exaggerated and their concern with practical considerations under-estimated."

The author has gone beyond other investigators into the sources, the pamphlet literature in particular, in her analysis of how Rousseau's name and ideas were used "when it was convenient." Yet there is more to be done, particularly in tracing the development of the thinking of individuals as the Revolution progressed, as McDonald has done in the case of Malouet. And of course the period after 1791 remains. She discusses this briefly at the end of the book, where she unfortunately falls into the temptation to generalize, deducing familiar conclusions from a slight sampling of the available sources. The subject of the role of Rousseau up to 1791 required the meticulous, inductive analysis she has given it; this is equally true for the period after 1791.

University of Arkansas

GORDON H. McNEIL

THE FRENCH REVOLUTION AND THE SCHOOLS: EDUCATIONAL POLICIES OF THE MOUNTAIN, 1792-1794. By *Robert J. Vignery*. (Madison: State Historical Society of Wisconsin for the Department of History, University of Wisconsin. 1965. Pp. xi, 208. \$3.00.)

THE author tries to clarify the role of the Jacobins in revolutionary educational policy and legislation, especially since their attitudes toward the schools and intellectual life have so often been subject to recriminatory debate.

The first quarter of the book describes the educational system and policies under the *ancien régime* and the stillborn educational plans produced by Mirabeau, Talleyrand, and Condorcet between 1789 and 1792. Thereafter the Jacobins come to the fore, and the author reviews the debate over each major educational plan presented to the Convention in the light of the political situation at the moment and of the factional interests the various plans seemed to reflect. In each plan it is evident that the Mountain wished to achieve two goals: intellectual development, which was commonly called *instruction*, and moral and political indoctrination, which was called *éducation*. There was disagreement only upon details or emphasis: whether education was to be free, whether it was desirable to have state and private educational institutions in competition, whether indoctrination should take place within the school or outside it, whether an intellectual elite should govern the direction of the lower schools, and so forth. It might surprise some modern critics of the Mountain to recall that most Jacobins were reluctant to give control of education to the central government.

This work shows that the issues in conflict were seldom clearly drawn and that fanaticism gave way to eclecticism in the educational legislation that received approval before *Thermidor*. This eclecticism makes the introductory chapters relevant, for we see the Jacobins originated no new idea or principles, but borrowed from a common stock traceable chiefly to Rousseau, the Encyclopedists, and the pre-1789 educational system.

Students of the Revolution will learn little new here; the book's value is that it provides a brief account of the subject in English.

Wesleyan University

STANLEY J. IDZERDA

MAISONS DE PARIS ET PROPRIÉTAIRES PARISIENS AU XIX^e SIÈCLE, 1809-1880. By *Adeline Daumard*. ([Paris:] Éditions Cujas. 1965. Pp. 284, 10 plates.)

THIS volume, the author's complementary thesis at the University of Paris, like her *Bourgeoisie parisienne de 1815 à 1848* (1963), is a pioneering work in French economic and social history—the first published study of urban property values and rents in France. Before its appearance historians' knowledge of the evolution of values and rents of Parisian buildings was little more than a number of ill-founded assumptions. Miss Daumard undertook to measure these changes systematically, to relate them to the country's general economic development, and to sketch the evolving social composition of the body of Parisian proprietors.

For her study of rents she selected six typical streets, and using as sources two property registers (*sommiers fonciers*), supplemented by tax records, notarial

archives, the *Petites affiches de Paris*, and other printed sources, she constructed for each street indexes and curves of rental incomes between 1809 and 1880, the initial and terminal dates of the two *sommiers*. Data on sales recorded in the *Petites affiches* enabled her to determine the average sale price of buildings sold in Paris in every fifth year between 1800 and 1880. Both these analyses revealed rents and market values rising steadily through the period. The ascending curves were interrupted in times of crisis, but they quickly resumed their upward movement. Rates of appreciation varied from quarter to quarter and from street to street, but no quarter and no street broke the general pattern. In the districts around the new Avenue de l'Opéra and the new Boulevard Saint-Michel, which the author studied to discover the influence of Haussmann's works, the rise in rents accelerated in the years after the completion of the improvements; rents from old buildings on old, less active streets, not only shared in the acceleration, but rose more rapidly than those from newly built structures on busy streets.

Until 1840 and perhaps later more than half of Parisian property owners were from the *petite* and *moyenne bourgeoisie*. By 1875 wealthier businessmen, proprietors living on the income from their property, professional men, and corporations dominated both in numbers and in the value of property they held. The rise in property values had made Parisian real estate increasingly inaccessible to men of modest means, a change that may have had important political consequences through its erosion of the commitment of the Parisian lower bourgeoisie to the existing political and economic system.

Two limitations make the author's conclusions less than definitive. They are based on samples that are but a tiny fraction of the vast complex of Parisian property, and many of her sources, as she points out, are not certainly reliable. The book has, nonetheless, moved its subject from the realm of guesswork to objective, quantitative study, and the results are enlightening in themselves and suggestive of paths to additional light.

University of Washington

DAVID H. PINKNEY

TALLEYRAND'S LAST DUCHESS. By *Françoise de Bernardy*. Translated from the French by *Derek Coltman*. (New York: Stein and Day. 1966. Pp. 348. \$6.50.)

DOROTHÉE of Courland is best known as the Duchess of Dino, the twenty-one-year-old niece by marriage whom Talleyrand took to the Congress of Vienna in 1814 to preside over his salon. Returning to Paris she reigned at the Rue Saint-Florentin as Talleyrand's mistress until his death in 1838. The scandals of her life vividly illustrate the extraordinary libertinism displayed by one section of European society. The Duke of Courland almost certainly was not Dorothée's father; she succeeded her mother as Talleyrand's mistress; during her twenty years with Talleyrand, who probably was the father of her daughter, Pauline, she carried on several other fleeting affairs; and after his death she turned to a passionate romance with Felix Lichnowsky, twenty years her junior, that lasted until his murder during the Revolution of 1848. Her last years at the great Château of Sagan were compounded of sadness, religiosity, and good works.

Many of the political evaluations are shaky. Talleyrand was not the real architect of the Bourbon Restoration of 1814; it cannot accurately be said that he "invented" the Orléans monarchy; and his admittedly important role in creating an independent Belgium was not unique. Save for the years from 1830 to 1834 when Dorothee shared the spotlight with him at the London embassy, her political life seems to have been insignificant. Yet the long letter that she wrote to Talleyrand in August 1834 begging him to give up all thought of further public office gives concrete and impressive evidence of her political acumen and her skill in argument. In a biography touching upon so many personalities the absence of an index is serious; footnotes are anecdotal rather than scholarly; and the proofreading, beginning with the first paragraph, is careless. The Duchess of Dino's fluent memoirs have long been in print, and many of her letters to Thiers, Molé, Barante, Vitrolles, and Madame Adelaide have also been published. The political historian will by preference turn to these. The social historian will, however, find in this volume an interesting picture, vividly presented with much genealogical detail, of a privileged society that long outlived the French Revolution.

Wheaton College

E. J. KNAPTON

LAVIGERIE, LE SAINT-SIÈGE ET L'ÉGLISE: DE L'AVÈNEMENT DE PIE IX À L'AVÈNEMENT DE LÉON XIII, 1846-1878. By *Xavier de Montclos*. (Paris: Éditions E. de Boccard. 1965. Pp. 661.)

THIS significant and intensive study of the development of Cardinal Lavigerie's thought and policy in relation to the papacy and the Church from his student days at St. Sulpice to the accession of Pope Leo XIII in 1878 will be followed by other works on Lavigerie's missionary activities and on the *Ralliement*. Montclos has investigated the papers of Lavigerie and other archival materials not used thoroughly by previous biographers or Church historians. Although the Vatican Archives are closed after 1846, he was able to obtain much of Lavigerie's correspondence with Rome. In an exhaustive bibliography he supplies valuable information on the location of documents related to his subject in public, private, and ecclesiastical archives.

Lavigerie was one of the most dynamic and vivid prelates of his day, but unfortunately his personality does not come to life in these pages because of the wealth of detail about the religious background of the times. Montclos endeavors to explain the reasons for the growing conservatism of his political and religious views. He believes that there was continuity in Lavigerie's thought and that he was a devoted servant of the Church, although he acknowledges that he was a politician and opportunist, was authoritarian in handling the affairs of his diocese, and never had any real interest in social justice for the masses. In his early days Lavigerie was opposed to authoritarian systems, favored a rational approach, declared that the Church must become reconciled to the modern world, and advocated separation of Church and state. He supported temporal power and infallibility for the papacy as advantageous for the Church. He was no intellectual, but preferred to act directly in relation to men and events and did so with considerable political and diplomatic skill. His mission to Syria following the Druse massacres

of the Christians revealed to him his true vocation as a missionary. He became convinced that France had a civilizing and religious mission in the world, which he worked to fulfill in his posts in Africa and in founding the White Fathers. The support that he received from Pope Pius IX for his missionary efforts helped to wean him from his liberal associates. Since he believed that all values compatible with Christian faith and morals should be respected and that the unity of the Church must be preserved if its apostolic work were to be maintained, he served as mediator at the Vatican Council of 1870 between the majority and the minority in the battle over papal infallibility, although he himself was aligned with the *Curia*. Lavigerie held that the welfare of the Church lay not in ecumenical councils but in making the sacred college more international in character, with foreign cardinals resident in Rome. He also stressed improvement and modernization of the education of the clergy. According to Montclos, Lavigerie's ideas found their fullest expression in the policies of Pope Leo XIII, who worked for the reconciliation of the Church and modern society, Roman unity, and Christian order.

State University College, New Paltz, New York

EVELYN M. ACOMB

THE MISSION OF VINCENT BENEDETTI TO BERLIN 1864-1870. By Willard Allen Fletcher. (The Hague: Martinus Nijhoff. 1965. Pp. xiv, 303. 31.30 gl.)

PROFESSIONAL diplomats are not generally remembered unless they happen to have been involved in a major international crisis or to have committed some kind of egregious blunder. Among nineteenth-century diplomats of the second rank, Vincent Benedetti is remembered because he did both. He participated in the talks with King William of Prussia that took place at Ems immediately before the Franco-Prussian War, and his earlier mistake of having left a compromising alliance proposal, written in his own hand, in Bismarck's possession weakened his country's position in neutral capitals once that conflict had started.

That Benedetti's reputation should rest on these two incidents is, in Willard Fletcher's opinion, unfair, and he has undertaken to correct this by writing a detailed account of the ambassador's service in Berlin in the period between the Danish and French wars. Based upon a staggering amount of research in archives in Paris, Brussels, The Hague, Luxembourg, and Vienna, and meticulous use of the printed materials, this thoughtful and illuminating diplomatic study is weakened only by the author's awkward and sometimes ponderous style, his penchant for using nouns as adjectives, and his excessive reliance upon the passive voice.

Benedetti was sent to Berlin in 1864 with instructions to discover the extent of the Prussian government's ambitions in Germany and, as far as possible, to exploit the differences between Prussia and Austria for the advantage of his own country. The first of these tasks he executed with distinction, his reports accurately predicting the course and objectives of Prussia's policy in the years that led to the war with Austria. In the second he was handicapped by the lack of coordination that marked French policy during the Second Empire and by the Emperor's habit of indulging in sudden sallies into the diplomatic arena without informing his ambassadors in advance. Thus, at a time when Benedetti was doing what he could to

encourage the negotiations for a Prusso-Italian alliance in the spring of 1866, Napoleon was advising the Italians to break off the talks; there were other examples of this kind of incoherence.

In the period that followed the Austrian war, Benedetti was clearheaded enough to see the emerging threat that Prussia represented to French interests but incapable of doing much to contain it. This was partly, Fletcher argues, because his efforts were defeated by the continued lack of direction in Paris, but even more because of the deviousness of Bismarck's tactics. As a mediator between Austria and Prussia at Nikolsburg, Benedetti had some success in modifying Prussian claims; as the spokesman for French projects of compensation, he was less effective and was led into those elaborate negotiations for a Franco-Prussian treaty at the expense of Belgium which Bismarck never meant to succeed and which terminated in failure in the spring of 1867. It was during these fruitless talks that Benedetti wrote the fatal document that appeared in the London *Times* in July 1870, a blunder only exceeded, as Fletcher points out, by the ambassador's disingenuous and easily refuted attempts to explain it away.

The third phase of Benedetti's mission extended from the Luxembourg crisis of 1867 to the outbreak of war, and here, in the author's view, his judgment was clouded by the desire for revenge against Bismarck. His lack of close contact with the Minister President had ill effects during the genesis of the Hohenzollern candidacy, and he left his government inadequately informed as the crisis blew up. On the other hand, his role in the Ems talks has been distorted, and the mistakes committed there were not of his own making but the result of explicit instructions from the Foreign Office.

On balance, Benedetti appears in this solid account as a respectable if not a distinguished representative of his country's interests, forced to operate between two highly unreliable statesmen, handicapped in the performance of his duties at times by inadequate or contradictory information from his home office and at others by instructions so rigid as to deny him opportunity for maneuver, but doing his job, nonetheless, in a conscientious manner.

Stanford University

GORDON A. CRAIG

LÉON BLUM: HUMANIST IN POLITICS. By *Joel Colton*. (New York: Alfred A. Knopf. 1966. Pp. xiv, 512, xiv. \$10.00.)

JOEL Colton is a meticulous researcher and a fine craftsman. In his political biography of Léon Blum, these two qualities are beautifully blended: none of the available evidence appears to have been overlooked, and the enormous mass of variegated material has been transmuted into a polished, richly tapestried, and absorbing narrative.

The author performs an impressive feat: he combines rigorous fairness with a deep-felt sympathy for his protagonist. He repeatedly calls attention to Blum's virtues as a human being, but just as often underscores his defects as a political leader. In assessing Blum's achievements during the era of the popular front, he divides his attention between solid successes and resounding failures. Colton's over-all appraisal is presented in the very last paragraph of the book: "Thus Blum had his

share of weaknesses and shortcomings as a political leader. His unbounded optimism, his passion for integrity, his faith in human beings, his desire for wide esteem, his eagerness to serve as conciliator, his sentimental attachment to the effectiveness of an enlightened public opinion, were not the best assets for leadership in any age; in his age, they were fatal flaws. He could not be lion and fox; he could be only a human being placing his high intellectual, humane, and moral qualities in the service of his ideals, his party, his country, and humanity." Few will question the soundness of this verdict.

Biography, even when limited to one albeit paramount dimension of a man's life, is, of course, a difficult art, and Colton meets the challenge superbly. This is without doubt the best portrayal in any language of Blum's career (or should we say ordeal?) as a statesman, and I have no hesitation in predicting that it will rank high in twentieth-century historiography.

Every historian is familiar with the paradox of public figures whose most admirable human qualities determine their political errors, and Blum is certainly a case in point. The very excellence of Colton's treatment of this paradox whets our desire to know a bit more about Blum's early, pre-1914 years—the period of his life that saw the formation of the virtues that were to clash so tragically with the demands of *Realpolitik*.

University of Chicago

S. WILLIAM HALPERIN

MARXISM IN MODERN FRANCE. By *George Lichtheim*. (New York: Columbia University Press. 1966. Pp. ix, 212. \$6.75.)

THIS slender but sinewy volume is a study of socialist and Communist theory and practice in France since the First World War. It focuses on the years since 1945, but gives full and thoughtful consideration to nineteenth-century Marxism in France and elsewhere, including its genesis, evolution, and eventual radical transformation after the October Revolution. There is a suggestive and incisive sketch of the native revolutionary tradition in France, pointing up the eclectic nature of a tradition that had to incorporate Blanqui, Proudhon, Saint-Simon, Marx, and many other elements. In the Leninist-Stalinist version of Marxism as practiced in France, at least until the death of Thorez in 1964, the author sees a heavy dose of Blanquism, especially in its concentration on elitism, the substitution of party for class, and the suppression of the pristine humanistic, libertarian, and voluntarist element in Marxism, which a contemporary generation has had to retrieve, partly by going back to Hegel, partly by returning to the writings of the young Marx. Even though the author has many important observations to make on a number of subjects, including democratic socialism, social Catholicism, Gaullism, economic planning, and so forth, the heart of the book is made up of three chapters analyzing the new revisionist Marxism ("neo-Marxism," or "post-Marxian socialism"). Hammered out in debate with the existentialists (Sartre, Merleau-Ponty, and others), with the Christian Socialists (Mounier and the *Esprit* group), and with other philosophical schools of thought, the new doctrines reached into the ranks of the party intellectuals themselves (at times largely because of the winds blowing from across the Alps), but were the accomplishment mainly of writers like Henri

Lefebvre who had broken with the movement. Although the author insists that much has to be discarded in classical Marxism, including proletariat worship, he sees Neo-Marxism as very much relevant to the "technocratic necessities of a new industrial and postbourgeois age," that is, an age that has seen the demise of the market economy and of classical liberalism—itself a quiet triumph for socialism.

Mr. Lichtheim presents us here with a solid and provocative exegesis, best suited for those who already know something of the theory and practice of the French labor movement in its Marxian and non-Marxian facets. Those who have kept up with the heady debates in *Les Temps Modernes* since the 1950's will have a running start, but the book has some important conclusions not only for historians but for economists, philosophers, and sociologists as well.

Duke University

JOEL COLTON

PARADES AND POLITICS AT VICHY: THE FRENCH OFFICER CORPS UNDER MARSHAL PÉTAIN. By Robert O. Paxton. (Princeton, N. J.: Princeton University Press. 1966. Pp. xi, 472. \$9.00.)

PROFESSOR Paxton has written a good book and a useful one, the chronicle of an army's attempt to stay alive, the subterfuges and compromises this entailed, the incidental interests it served, the final failure it could not avoid, the traces it has left in the personnel and thinking of France's postwar forces.

The Armistice Army, purged rump of a defeated force, served the self-interest vested in its perpetuation and the *ordre moral* Vichy wished to restore. Much has been claimed for it; little was achieved. While it lasted, from 1940 to 1942, tightening of numbers favored not efficiency but conformity. Vaunted innovations proved makeshift and petty, directed less at effectiveness in battle than at propaganda. Not progress but backwardness, not initiative but tradition ruled over its shrunken, decorated ranks. Armament was laughable: cavalry regiments returned to bicycles and horses; illegal arms of doubtful quality were hidden in quantities insufficient to make much difference; secret plans were resolutely based on improbable contingencies; underground activities irrelevant to the course of war or the glory of the service provided substitutes for action; wily maneuvers played Germans against Italians with the result of substituting the former for the latter and, occasionally, of setting the French to fire on other Frenchmen if not, more palatably, on English or American forces. By the end of 1942 the Armistice Army, ingloriously suppressed in France by the German occupant, reaped its rewards in North Africa whence it could set off again, re-equipped by allies on whom it had lately fired and officered by men forgetful of allegiance to Pétain, to the reconquest of France and its position in it.

All this and more Paxton's lucid narrative recounts, more matter-of-factly than my compressed version of his findings. His solid and objective story, based on vast research, draws heavily on German sources, on the slenderer French evidence available, and on interviews. It is to be hoped that a French translation may soon make it available where it will be read with less detachment and, almost certainly in some quarters, with less pleasure, too.

University of California, Los Angeles

EUGEN WEBER

THE ST. PIERRE AND MIQUELON *AFFAIRE* OF 1941: A STUDY IN DIPLOMACY IN THE NORTH ATLANTIC QUADRANGLE. By Douglas G. Anglin. (Toronto: University of Toronto Press. 1966. Pp. xvi, 219. \$6.00.)

THE tiny French islands of St. Pierre and Miquelon have historically been a political storm center far out of proportion to their size. In his excellent study, Professor Anglin discusses the political and diplomatic problems leading to and growing out of the take-over of the islands on December 24, 1941.

Although the island population strongly opposed Marshal Pétain's growing collaboration with the Nazis after the defeat of France, the administrator of the islands, Count Gilbert de Bournat, and Monseigneur Poisson, the apostolic prefect, did their utmost to keep the islands under Vichy's control. As Anglin shows, the strategic importance of the islands assumed therefore an extraordinary international significance. Under direct or indirect Nazi control the islands could have been used to menace the security of the Northwest Atlantic region, to attack Allied ships, and to use St. Pierre's short-wave radio transmitter for the purpose of communicating Allied ship movements and meteorological reports to the Nazis. While Great Britain, Canada, and, particularly after Pearl Harbor, the United States had obviously an interest in preventing these possibilities, De Gaulle was furthermore determined to liberate the islands in order to enhance the power and prestige of Free France. As he saw it, the future of France would largely depend on bringing the French Empire into the war.

Unlike Churchill, Cordell Hull was strenuously opposed to any "drastic" action regarding the islands. He was afraid that any clash between Vichy and De Gaulle over St. Pierre and Miquelon would produce undesirable repercussions in Europe and Africa. It would, moreover, upset Roosevelt's recent understandings with Latin America and Vichy with respect to the status of the French possessions in the Americas. De Gaulle, on the other hand, looked upon a take-over of the islands as an "internal" French affair over which the United States had no right to exercise a veto. After his successful landing, Admiral Muselier echoed De Gaulle's defiant attitude when he declared: "I will resist any navy of any power. . . . Here we are and here we shall remain."

In his brilliant analysis of Hull's reaction to these developments, the author stresses the incredible ineptness the Secretary of State displayed in this episode. De Gaulle resented Hull's reference to the "three so-called Free French ships at St. Pierre-Miquelon" as much as Canadians took exception to Hull's impolitic intimation that he expected them "to restore the *status quo* of these islands." In the author's estimate, Hull's failure to see this affair in its proper perspective did much damage to his reputation and influence.

This is a well-researched, readable, and enlightening book. Based on official documents, memoirs, private correspondence, and personal interviews, this study makes a valuable contribution to the history of World War II. Readers will, moreover, find in it significant background material concerning De Gaulle's contemporary attitude toward the United States.

Rutgers University

HENRY BLUMENTHAL

THE FRENCH ARMY IN POLITICS, 1945-1962. By John Steward Ambler.
 [Publication of the Mershon Center for Education in National Security.]
 ([Columbus:] Ohio State University Press. 1966. Pp. x, 427. \$6.50.)

THE role played by the French Army from 1958 to 1962 was a reprise of De Gaulle's defiance of "legitimacy" in the tragic days of defeat in June 1940. De Gaulle, more the noble Antigone than Joan of Arc, unwittingly prepared the way for such Epigoni as Generals Salan, Challe, Jouhaud, Zeller. Invoking the transcendent "moral law" in the national interest is a risky game that more than one can play, as the French military feudatories in Algeria have recently demonstrated.

Professor Ambler has written an admirable account of the French Army's straying from the path of obedience. In an excellent introduction he explains how the French officers, after the Napoleonic imperium, became an obedient "bureaucratized nobility." Facilitating this process was a relative absence of serious threats of security until the Franco-Prussian War. In the early decades of the Third Republic, neocolonialism afforded a "safe" outlet for the animal spirits of restive officers who, like General Boulanger, might have been tempted to enter politics had they remained in the metropolis. The army of any nation will be inclined to move into a political vacuum, but for all the giddy succession of cabinets in nineteenth- and twentieth-century France, the nation was fundamentally stable, enjoying a tacit consensus of the mass of property-owning peasants and the bourgeoisie. Only the slowly emerging urban laboring class was alienated and responsive to Communist appeal. The depression of the 1930's and the concomitant rise of the fascist leagues weakened the social consensus and bipolarized the army between fascist-minded officers and a rank and file disposed to follow the Communist line during the phony war.

Ambler traces the dilemma of the military who, after the debacle of June 1940, had to choose between De Gaulle and resistance and Pétain, the legal authority, and defeat. He gives a masterly analysis of the misreading of Mao Tse-tung's doctrine of *la guerre révolutionnaire*, which the frustrated officers thought they had learned from the Viet Minh in Indochina, and which they were determined to apply in Algeria. Thinking that De Gaulle would help them "save Algeria"—their last great opportunity overseas—they threatened a *coup d'état* in June 1958 and put De Gaulle in power, only to have him free Algeria and black Africa. Generals and colonels condoned the *pidé noir* insurrection in January 1960, and, that failing, they led their own in April 1961. But there was no "weak government" in 1961 as there had been in 1958, and De Gaulle was supported by the metropolis and by history. The latter-day Antigones ended up behind bars.

This is a splendid book with a voluminous bibliography and a mine of well-marshaled factual information full of meaning for anyone interested in France, in civil-military relations, or, more currently, for any American concerned over Vietnam.

University of California, Los Angeles

JERE CLEMENS KING

THE SPANISH SEABORNE EMPIRE. By J. H. Parry. [The History of Human Society.] (New York: Alfred A. Knopf. 1966. Pp. 416. \$6.95.)

THE key to the contribution this splendidly written volume makes is the title *Seaborne Empire*, rather than the series title "The History of Human Society," for maritime matters and imperial viewpoints are stressed more than the development of society in a new world. Professor Parry includes chapters on Indian cultures, the society of conquest, and the American-born Creoles, but his most effective narration concerns the dramatic feats of Cortes and Pizarro, the intricate patterns of European diplomacy influencing Spain's action overseas, and what he calls "The Maritime Life-Line," a brilliant account of oceanic trade. Even here, the emphasis remains on the economic and strategic significance of these sea routes for the Empire, with little attention given to the human elements.

Compact chapters follow on the development of legislation for the Indies and the doubts that accompanied the laws relating to the natives, the melting away of the Indians and the results of this catastrophe, the economic effects of American silver on Spain and Europe generally, European attacks on the Spanish colonies, and the eighteenth-century reforms wrought by the Bourbons. The concluding sections on the wars for independence and "The Aftermath of Empire" come as a kind of anticlimax, for they cover large and complicated topics in very brief compass.

Mexico and the Caribbean receive more detailed treatment throughout than the vast stretches of the viceroyalty of Peru, and this leads to a certain unevenness. Scientific expeditions and cultural developments, except architecture, are skipped over lightly or omitted altogether; such figures as Sor Juana Inés de la Cruz and Carlos Sigüenza y Góngora are not mentioned, though we are told that "the Creole male tended to be more interested in horses than in ideas." Some of the many other obiter dicta scattered throughout the volume raise more serious questions. We probably do not know enough yet to claim that "it is not a great exaggeration to say that the discovery of Potosí was one of the turning points in the history of the Western World," and other generalizations, especially on the insufficiently studied seventeenth century, have not yet received much documentary support.

This volume lacks the intellectual bite of Diffie's *Latin American Civilization*, does not give the picture of institutional growth of Haring's *Spanish Empire in America*, and is less bibliographically current than Konetzke's work in the "Fischer Weltgeschichte." Though Parry incorporates the results of some outstanding recent research, such as the contributions by Borah and the Chaunus, his bibliography omits many other important works, particularly by Spanish American scholars whose archival investigations have done so much during the last quarter century to determine regional variations in the Empire. Few of their journal articles are cited; such historians as Eduardo Arcila Farías, Juan Friede, and Alvaro Jara are not listed. The numerous items by Mexican scholars in the bibliography recently published by *Historia mexicana* (XV [Nos. 59-60, 1965-66]) were apparently not consulted.

The Spanish Seaborne Empire, therefore, may be recommended as an attractive broad-brush treatment for the general reader, but the student who seeks a

comprehensive analysis based on the rich material available is more likely to go to Charles Gibson's *Spain in America*.

Columbia University

LEWIS HANKE

SPAIN, 1808-1939. By *Raymond Carr*. [Oxford History of Modern Europe.] (New York: Oxford University Press. 1966. Pp. xxix, 766. \$12.50.)

THE REVOLUTION OF 1854 IN SPANISH HISTORY. By *V. G. Kiernan*. (New York: Oxford University Press. 1966. Pp. viii, 266. \$7.70.)

Mr. Carr's volume is an encyclopedic political and social history, interspersed with thoughtful interpretations. No one else, Spaniard or foreigner, has read so deeply and reflected so intelligently on nineteenth- and twentieth-century Spain. Because of its minute inclusiveness, the book does not make easy reading. The narrative is sometimes difficult to follow, and the author is fond of long sentences adorned with numerous dependent clauses and occasionally confusing punctuation. But the careful reader will find many important conclusions quietly phrased as they develop out of the wealth of evidence. Carr finds, for example, that opposition to entail was not so much hatred of aristocratic landownership as it was "a crusade for capital investment in land and agricultural efficiency." He finds that after the first Carlist War the failure of civilian politicians brought the generals reluctantly to assume political leadership. He shows how the parliamentary system of the 1840's was already like that of the Restoration in its essentials. He points out that the middle class was not so weak in numbers and in wealth as has been repeatedly assumed; its political ineffectiveness resulted rather from "the disparity and localism of its interests." His detailed knowledge of a long period enables him to make many significant comparisons. Thus in connection with the political failures of the twentieth century he states that "intellectuals could become civil servants in the eighteenth century; formed in the individualistic mould of '98 they could not become party men in the twentieth." Similarly he points out that just as in the Carlist Wars military rule began at the local level, so the dictatorship of Primo de Rivera began with his local rule as captain general in Catalonia. Although the author deliberately concentrates on internal affairs, he suggests many illuminating analogies, especially with French developments. Thus he emphasizes the humble social origins of eighteenth-century Spanish bishops, in contrast to the aristocratic monopoly of high Church posts in prerevolutionary France. He shows how the *Moderados* looked to the model of the July Monarchy, and how Bravo Murillo was impressed by the methods of Napoleon III. He compares the accomplishments and weaknesses of the Azaña government in 1932-1933 with those of the Blum government in 1936. Carr has a faculty for summarizing clearly the results of the best scholarship of the past half century, as in his discussions of Krausism, Catalan and Basque nationalism, economic development, and the anarchist and Socialist movements. His footnotes will be invaluable to other scholars for decades to come. His judgments are coolly made, but include an underlying sympathy with the people about whom he writes.

Mr. Kiernan gives a useful chronological-political history of the revolution of 1854. He wishes to attribute distinctive significance to this particular *biennio* by

claiming that it was the starting point for a century of agitation by landless peasants and industrial workers. But his own detailed account of 1854 will leave the reader feeling that in fact the revolutions of 1820, 1837, and 1868 involved larger issues and a higher proportion of the populace than did the events of 1854-1856. The contents of the book do not have the balance of true scholarship. There are many breezy generalizations from which it would be hard to extract real meaning. On the other hand there are two separate footnotes for two not terribly significant descriptive phrases about Isabella II quoted from two English travelers of the 1850's. Scholars will find much new information on the political infighting among Isabella, Espartero, O'Donnell, and Olózaga. But the book is not noteworthy for interpretation and perspective.

University of California, San Diego

GABRIEL JACKSON

HET STERFHUIS VAN WILLEM VAN ORANJE. By *P. Scherft*. [Leidse Historische Reeks, Number 12.] ([Leiden:] Universitaire Pers Leiden. 1966. Pp. 342. 32.50 gl.)

THE assassination of William the Silent on July 10, 1584, was a disaster for his family as well as for his people. The Prince of Orange left behind him a young widow and no less than twelve children from a total of four marriages. There was no will, only the project of one. And who could say what the estate consisted of, when so many of the properties were in the hands of the enemy? Not until peace came would the States-General be in a position to meet their enormous financial obligations to the House of Orange in a completely satisfactory way. Another difficulty was that the oldest son, Philip William, was being held prisoner in Madrid. As a result of all these complications the estate was only divided after the Truce of 1609. By that time the younger sons, Maurice and Frederick Henry, had obtained the lion's share of the inheritance. Maurice treated his brothers badly and his sisters shamefully. Since he was Stadholder of Holland, Zeeland, and eventually of three other provinces as well, he could bend the courts to his will or even ignore them. Luckily the tripartite division was not to be permanent. Philip William had no children by his marriage, and on his death in 1618 his share passed intact to the bachelor Maurice. Seven years later when Maurice died Frederick Henry acquired the entire complex of estates.

Dr. Scherft takes the story down to 1609 and gives a summary of the events of the next sixteen years. Apparently he was hindered by a number of gaps in the surviving records, so that it is nowhere clear just what the estate comprised, nor what the revenues were. Thus his book will be of more use to the political than to the economic historian. A few cuts in the discursive prose would have made room for tables, a genealogical chart, and perhaps a map. Although the author has not solved all his structural problems, or asked all the right questions of his material, he has made a useful contribution to the history of the House of Orange and particularly to that of Maurice. A summary of the argument is provided in French.

University of North Carolina

STEPHEN B. BAXTER

DE OORLOGVOERING TER ZEE IN JOURNALEN EN ANDERE STUKKEN. Edited by *J. R. Bruijn*. [Werken uitgegeven door het Historisch Genootschap (Gevestigd te Utrecht), Third Series, Number 84.] (Groningen: J. B. Wolters, 1966. Pp. 248.)

THE closing three naval battles of the third Anglo-Dutch War (1672-1674), which frustrated the design of the French and British to invade Holland from the sea, helped secure the independent existence of the Dutch Republic for at least another century and thus constitute a prime example of the influence of sea power upon history. Moreover, the Dutch fought all three engagements under the personal direction of one of the great practitioners of naval warfare, Admiral de Ruyter. The battles fought off the southern Dutch coast near Schoneveldt on June 7 and June 14 and off the island of Texel on August 21—all in 1673—have therefore frequently attracted professional naval historians, among them J. C. de Jonge, A. T. Mahan, J. C. M. Warnsinck, and E. Coenen.

Though the history of these engagements has been relatively well explored, its primary sources have until recently not been readily accessible. The present publication fills this gap, providing a Dutch counterpart to the *Journals and Narratives of the Third Dutch War* by Roger Charles Anderson (1946). Reproduced in full for the first time are De Ruyter's own journal (April 29-September 25, 1673) and the journal kept by his son, Engel de Ruyter, who served under his father in all three engagements (March 15-October 1673).

In addition to these two eyewitness accounts, the present collection of documents includes a narrative reconstructed by the son of Admiral Sweers in 1729 with the aid of documents left him by his father, who died in the Battle of Texel. As an appendix the present volume prints five letters written by De Ruyter to his two married daughters and an official list of ships that participated in the Battle of Texel.

With the exception of the ship's list, which corrects the one published by Anderson, the documents reproduced here are likely to be of greater interest to the general reader than to the specialist. While De Ruyter's journal does not at all reveal his tactical thinking, it does illuminate the man himself and his relations with the political leaders of his day. With the fleet operating in home waters, the Prince of Orange and the members of the States-General kept an astonishingly close rein on naval strategy.

The texts have been edited and annotated with care. The editor has written a useful, scholarly introduction and has provided a helpful glossary, a bibliography, and indexes of persons, ships, and places.

Washington, D. C.

B. H. WABEKE

DE ARBEIDSINZET: DE GEDWONGEN ARBEID VAN NEDERLANDERS IN DUITSLAND, 1940-1945. By *B. A. Sijes*. [Ministerie van Cultuur, Recreatie en Maatschappelijk Werk, Rijksinstituut voor Oorlogsdocumentatie. Monografieën, Number 11.] (The Hague: Martinus Nijhoff, 1966. Pp. xi, 730.)

Sijes's new book is a "definitive" study of the German labor draft in the Nether-

lands during the Second World War. It is the product of more than ten years of painstaking labor and complements and in many ways supersedes A. J. H. Bauer's earlier work *De openbare arbeidsbemiddeling gedurende de bezettingstijd 1940-1945* (1948). The forty-page English summary makes the substance of the book available to the reader not familiar with the Dutch language.

Sijes's special contributions include his well-documented demonstration that the long-standing desire of Dutch government officials to achieve a more efficient organization of employment services was a major factor inducing such civil servants as Acting Secretary-General Verwey to cooperate with the Germans. He also shows that Speer's determined policy to transfer German war production to the occupied territories, rather than the opposition of Dutch officials or of the population, was, until the summer of 1944, the single most important impediment to even more massive deportations. The chapter on Dutch workers in Germany constitutes a contribution to the history of forced labor during the war. In a wider sense, Sijes's book provides one more detailed illustration of the deleterious effect on the German war effort of policy conflicts within the Nazi hierarchy.

Sijes's judgments of the conduct of his countrymen are balanced and fair, treading the middle ground between wartime emotionalism and the new revisionism which explains and justifies everything. He is critical of the collaboration of top civil servants while recognizing their motives. He acknowledges that patriotic motives played only a minor part in determining the attitudes of potential draftees and that there was relatively little objection on grounds of principle to work in support of the German war effort in Dutch factories. Although by the nature of its conception not as dramatic as Sijes's earlier monographs on the February strike of 1941 and the labor raids of 1944, the present work constitutes his most important contribution to the history of the Second World War.

Pitzer College

WERNER WARMBRUNN

OTTO THOTT'S UFORGRIBELIGE TANKER OM KOMMERCIENS TIL-
STAND: ET NATIONALØKONOMISK PROGRAMSKRIFT FRA 1735.
By *Kristof Glamann*. ([Copenhagen:] Københavns Universitets Fond til Til-
vebringelse af Læremidler. 1966. Pp. 138.)

Otto Thott (1703-1785) belonged to a distinguished Danish noble family and, like others of his class, completed his education by a long period of travel and study abroad. Upon accepting office at home, in the middle of a period of economic depression comparable, in relative seriousness, to that of two centuries later, Thott wrote an official memorandum on the state and origin of commerce. Perhaps his ideas were contestable, but this edition of his program, hitherto only available in manuscript in the Danish *Rigsarkiv*, is not.

Taking up the second and shorter part of the book, the memorandum is concerned with Danish agriculture, manufactures, and commerce. With little theorizing, Thott described the situation in 1735 and then suggested remedies for problems. Danish agriculture, he argued, was depressed, with prices on necessary imports remaining high while prices on Danish products were low. Competition from Schleswig, then a part of the domestic economy, had been ruinous. A migration to

Copenhagen from the countryside left the farmers with a shortage of manpower and a general shortage of money. Thott suggested that tax relief, protection against Schleswig, and the encouragement of individual incentive, energy, and thrift would have led to greater domestic consumption. Storage facilities and a minimum price on grain, more trade with Norway, commercial treaties with other countries, and import restrictions would also have helped. The labor shortage could have been solved by keeping youths on the farms, by settling indigent children from Copenhagen in the country, and by immigration. Luxuries should have been restricted and foreign goods bought directly from the seller and carried in Danish ships, with the exporter also being a buyer of Danish goods. Domestic manufactures could have been encouraged in such ways as, for example, by promoting the practical sciences within Danish education. Commerce, finally, was described as the axle between the wheels of agriculture and manufacturing, allowing them to roll. Active and enterprising merchants were needed who realized that "infallibly the first and most certain economic rule for becoming rich is to save."

Glamann's germane and well-constructed analysis, making up the first part of the book, concentrates on Thott's views on agriculture and commerce. He provides the background and critical apparatus necessary for an understanding of the treatise in its time and place, and he has performed his task well.

University of California, Riverside

ERNST EKMANN

FOR JYSK OG FOR DANSK HISTORIE: JYSK SELSKAB FOR HISTORIE, SPROG OG LITTERATUR, 1866-1966. [Skifter udgivet af Jysk Selskab for Historie, Sprog og Litteratur, Number 14.] (Aarhus: Universitetsforlaget. 1966. Pp. 147.)

THE Society for the History, Language and Literature of Jylland (Jutland) has issued, under the editorship of Vagn Dybdal, an anniversary issue that combines three lines of development in its few pages. Besides the editor, ten others contribute essays varying in length from four to twenty pages.

A section of some thirty pages dealing with the background and formation of the society by two writers is followed by five brief biographies of its editors from 1866 to 1937. These are all valuable for the history of local research and publication; in two of them, on Villads Christensen by Tage Kaarsted and on Sigvard Nygard by Johan Hvidtfeldt, there is that shimmer of sympathetic understanding too often lacking in such brief biographical sketches. The complete reconstruction of the society after 1930 and its conversion into a national body occupies the last four sections, some forty-six pages. This brought growth in membership, but some divergence from the original aims and ideals. Yet it has managed to keep some portion of its first intent and has resisted rather well the increasing pressure of Copenhagen with the danger of the capital swallowing up the hinterland. The print is clear, the binding good, the pictorial reproduction satisfactory, and the proofreading outstanding. This is not a story of great historical work, but of good, hearty labor.

University of Southern California

FRANCIS J. BOWMAN

DIE OSTKARELISCHE FRAGE: DIE ENTSTEHUNG EINES NATIONAL-ALLEN EXPANSIONSPROGRAMMS UND DIE VERSUCHE ZU SEINER VERWIRKLICHUNG IN DER AUSSENPOLITIK FINNLANDS IN DEN JAHREN 1918-1920. By *Mauno Jääskeläinen*. [Studia Historica, Volume VI.] (Helsinki: [Suomen Historiallinen Seura.] 1965. Pp. 299. Fmk. 12.)

THE Finnish declaration of independence on December 6, 1917, had barely been translated into independence actually achieved (by the victory of the legal government in the war precipitated by the Red revolt in January-May 1918) when relations with Communist Russia became disturbed by the east Karelian question. The question had emerged, in the first instance, because the Finns hoped and attempted to bring their Karelian kinfolk, living beyond Finland's eastern boundary in Russia, within the borders of the Finnish Republic. Failing to persuade the new masters of Russia that the principle of national self-determination should be applied to the east Karelians and that east Karelia should be ceded to Finland, the Finns ultimately succeeded, or thought they had succeeded, in inserting into the Dorpat peace treaty of October 20, 1920, provisions sufficient to safeguard the language and cultural "nationality interests" of the Karelians. The effort failed and, in retrospect, was bound to fail. Later Finnish endeavors, in 1921-1923, to secure the east Karelian's "nationality interests" by obtaining action of the League of Nations in their behalf likewise met with no success and merely show how unrealistic Finland's policy in the matter was.

Dr. Jääskeläinen's able study covers the crucial years 1918-1920. His description of the problem and especially of the means and measures whereby the Finns attempted to carry their expansionist endeavors to a successful conclusion is clear and persuasive. Two score pages are devoted to an illuminating summary of the nationalist-romanticist conceptions current in the country after the emergence of the modern Finnish nationalist movement over a century ago. The problem of east Karelia during World War I is discussed at length, the events of 1918 (including the maneuverings at the Peace Conference) are adequately described, and the events leading to the Dorpat peace treaty are admirably delineated. The discussion is objective throughout, which is merely one way of saying that the author is free from the sentimental encumbrances that frequently have characterized Finnish discussions of the east Karelian problem and Finland's foreign policy objectives after 1918.

The Dorpat peace treaty provided, among other things, that the Finnish volunteer forces that had advanced into east Karelia be withdrawn. It also stated that the two provinces involved "will be reunited with the Russian State and will form the autonomous East Karelian area that enjoys the rights of national self-determination." The inhabitants of the two provinces were free to move within a year, with their property, to Finland. These and other provisions of the treaty did not prevent future friction. The result was that Finland was regarded by the Soviets as an aggressor intent on territorial gain, while the Finns were inclined to view their eastern neighbor as a repudiator of the principle of national self-determination whose treaty commitments could not be taken literally. By 1939 it had become

clear, however, that the Finnish case was weak and could not, under any foreseeable circumstances, be successfully pursued.

Columbia University

JOHN H. WUORINEN

AGRIPPA AND THE CRISIS OF RENAISSANCE THOUGHT. By *Charles G. Nauert, Jr.* [Illinois Studies in the Social Sciences, Number 55.] (Urbana: University of Illinois Press. 1965. Pp. vi, 374. Cloth \$8.00, paper \$7.00.)

AGRIPPA von Nettesheim (1486-1535) was a prototype of Faust, the restless pursuer of human knowledge and human power who lived his life between optimism and despair. Learned in theology and in many branches of philosophy, like other Renaissance Faustians he looked to occultism for the key that would fit all doors. Practitioner as well as theorist, he was famous in his day as a magician and astrologer.

As a Renaissance magus of the first rank Agrippa warrants the detailed treatment Professor Nauert gives him in this book. Belief in magic and occultism, as many scholars have been pointing out, was an intellectually respectable and nearly ubiquitous aspect of the thought of the period. As a point of view from which Renaissance thinkers looked at their world and man's place in it, it can no longer be dismissed merely as arrant superstition or charlatanism, nor shunted aside as an irrelevant diversion on the path toward modern science. But Agrippa had another side, and this makes him doubly interesting. Not only did he come to raise serious doubts about occultism (while continuing to practice it), but he composed one of the most widely read skeptical treatises of the age, *De incertitudine et vanitate scientiarum* (1526), in which he questioned the ability of the human intellect to reach any truth whatsoever. What relation these two sides of Agrippa's thought bore to each other is a difficult but important problem, not peculiar to Agrippa.

Nauert tackles his subject with industry and courage. If he sometimes expresses himself as though he minimizes the amount of attention that has recently been given to Agrippa and to the whole subject of Renaissance occultism, he is, nevertheless, well informed on what has been done, and he cites recent scholarship frequently. He gives us much more information on the life of Agrippa than we have had before, adding considerable detail to the emerging picture of a new kind of professional savant, strongly drawn to the traditional fields of philosophy and religion but unable to fit himself into the traditional social roles of cleric, schoolman, or even humanist. He deals, on the whole successfully, with the questions of Agrippa's connections with secret societies and his relation to the Protestant Reformation. Most of all Nauert is interested in the problem of Agrippa's controversial place in intellectual history. He follows Cassirer, Garin, and others who have insisted that occultism and skepticism were two facets of a single movement: the shattering of the medieval bases of human knowledge and the attempt to reconstruct a new approach. The solution undoubtedly lies in this direction, although it does not follow that Agrippa was moving, however uncertainly, toward the methodology of modern science, as Nauert maintains. Here he seems to disregard his own sound principle, that magic ought to be studied "as a historical phenomenon in its own right, whether or not it led western mankind toward the great

scientific advances of the seventeenth and later centuries." In keeping to this main issue, Nauert has succeeded in making an important contribution.

Rutgers University

DONALD WEINSTEIN

THE GERMAN MILITARY ENTERPRISER AND HIS WORK FORCE: A STUDY IN EUROPEAN ECONOMIC AND SOCIAL HISTORY. Volume II. By *Fritz Redlich*. [Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte, Beihefte, Number 48.] (Wiesbaden: Franz Steiner Verlag GmbH. 1965. Pp. viii, 322. DM 48.)

THIS concluding volume of a fundamental work in modern European military and social history is based on massive research in a wide variety of sources, and its style is reasonably clear and quite free of social science jargon. The volume begins with an analysis of the military entrepreneur's business from 1650 to 1800, his income, his risks, and the reasons for his decline. "What Napoleon extinguished was *princely*," not private, "entrepreneurship, and, after his downfall and the introduction of universal military service, . . . no room was left for a revival" of what had become a thoroughly "vicious institution." Two chapters deal with the origins, mobility, motivation, professionalization, and domestication of the officers, whether they were military contractors or not, and three with the origins, lives, and rewards of the common soldier. These were the years in which the "uprooted, roving mercenary" of the Thirty Years' War and much earlier wars was replaced by a worker "who served his whole life in the same army, unless he deserted or in rare cases was discharged at the end of a war," at real wages that declined after about 1750 from those of an artisan to those of a day laborer.

The two volumes have a single index of names and a single bibliography. Neither (at seventeen dollars in hard covers and twelve dollars in paper covers) is cheap, even by American standards. Since both are essential to any serious student of the military history of a fairly long period, a less expensive summary of Professor Redlich's conclusions—something like Michael Roberts' superb *The Military Revolution, 1560-1660*—would be most welcome. The works of Roberts, Craig, Wedgwood, Nef, Dorn, Rothenberg, and other American and English historians who have dealt with this period are not mentioned in the bibliography. While they were not necessary to the author's research, it would have been most interesting to have his comments on them. He has, in short, done so expert a job that he "owes" the general reader a shorter and cheaper survey of warfare in this period.

Duke University

THEODORE ROPP

DE LA FRANC-MAÇONNERIE MYSTIQUE AU SACERDOCE: OU LA VIE ROMANTIQUE DE FRIEDRICH-LUDWIG-ZACHARIAS WERNER (1768-1823). By *Louis Guinet*. [Publication Number 3.] (Caen: Association des Publications de la Faculté des Lettres et Sciences Humaines de l'Université de Caen. 1964. Pp. 246.)

IN writing this biography, Professor Guinet has excellently described and analyzed the tormented life of the German romantic dramatist and poet, Friedrich Ludwig

Zacharias Werner (1768-1823). Torn between the duality of sensuousness and carnal pleasure on the one hand and ethereal, Neoplatonic love on the other, Werner's life stands as a romantic archetype. One impulse led him to the theater, actresses, prostitutes, and three marriages, while the other led him to mysticism, Freemasonry, and the Catholic Church. His plays and poetry testify to his dual nature and internal struggle. He was a spiritual nomad who finally sought certainty in Catholicism. He entered the Church in 1811 and was ordained a priest in 1814, but he never found the peace he sought, only further doubts and pitfalls.

Guinet has convincingly explained Werner's problems in psychoanalytic terms. The dramatist was dominated by his pietistic and neurotic mother, who, at times, believed that she was the Virgin and that her son was Jesus. The susceptible boy soon accepted his sacred mission and rejected the Enlightenment teachings of his father and his professors at the University of Königsberg. He saw Freemasonry, like Catholicism, as a mystical means to achieve moral purification. His numerous affairs and marriages, according to Guinet, merely constituted Oedipal infidelities committed against his mother. His plays were marked and sometimes marred by this mystical and pietistic bent.

What makes this work especially valuable is Guinet's careful research into Werner's relationship to contemporary writers, most notably Goethe and Mme. de Staël. Guinet has quite convincingly shown how Werner's visits to Weimar in 1808 and 1809 influenced Goethe's writing of the last scenes of *Faust*. Unfortunately, however, while scrupulously examining the details of "la vie romantique," the author has failed to place his subject in the broader context that he deserves. The reader is left to assess the full stature of Werner as a writer and a literary innovator without Guinet's assistance. It is almost by chance that the author relates how Werner's drama commemorating the anniversary of his mother's death, *Der vierundzwanzigste Februar*, created a new dramatic genre, the *Schicksalsdrama* or drama of fate. To relate in detail Werner's influence on Goethe is not to reveal his full artistic importance.

Many biographies of Werner have preceded this one, the first being that of Werner's friend, Hitzig, in 1823. In fact, most of the present study is derived from the author's earlier study, *Zacharias Werner et l'ésotérisme maçonnique* (1962). Guinet's works have added a unique, analytic description of Werner's remarkable life to this literature.

University of Wisconsin

STERLING FISHMAN

FINANCIAL INSTITUTIONS AND INDUSTRIALIZATION IN THE RHINELAND, 1815-1870. By *Richard Tilly*. (Madison: University of Wisconsin Press. 1966. Pp. viii, 197. \$6.50.)

THIS competent account of the history of financial institutions in the Rhineland between 1815 and 1870 stresses their role in the economic development of this important industrial region. Relying on Rhenish provincial and local records, including the archives of the Cologne banking house of Oppenheim, Professor Tilly analyzes in detail the financial institutions and practices of this period, but not

without gauging their significance from the viewpoint of a modern economist. These economic evaluations are often enhanced by quantitative estimates that provide an over-all configuration. This penetrating analysis of widely scattered sources constitutes the lasting contribution of this work.

In a broader sense, Tilly believes that causal relationships proceed from economic to financial development and, in this case, without the benefit of any significant government aid. Thus, his monograph demonstrates that Rhenish bankers met successfully within the restrictive framework set by Prussian financial policies the increasing demands made upon them for industrial capital by private enterprise. In this connection, students of institutional history will be interested in the development of mixed banking in the Rhineland, a device so characteristic of later German practice.

Tilly does not try to investigate the availability of other capital sources for industrial development; nor does he concern himself with the growth of state-owned industries, questions one wishes he would make the subject of his next study. Nor does the work attempt to analyze Prussian economic and fiscal policies as such, so that in my opinion the author's conclusion that "the intent, if not the effect, of Prussian monetary and banking policy toward the Rhineland before 1870 was to restrict the latter's industrial development" needs further study and clarification. Such, largely self-imposed, limitations should in no way detract from the value of this work.

Library of Congress

ARNOLD H. PRICE

DER DEUTSCHE REFORMVEREIN: GROSSDEUTSCHE STIMMEN UND KRÄFTE ZWISCHEN VILLAFRANCA AND KÖNIGGRÄTZ. By *Willy Real*. [Historische Studien, Number 395.] (Lübeck: Mattiesen Verlag. 1966. Pp. 228. DM. 23.20.)

THE author of this monograph discusses the attempt to organize and hold together *grossdeutsch* opinion—one scarcely dares to use the word "forces"—during the critical years 1859–1866. He has had access to the unpublished papers of leaders in the effort, Heinrich von Gagern, Gustav von Lerchenfeld, Oskar von Wydenbrugk, and August Reichensperger, and those of the *Deutscher Reformverein* itself, and he has explored the published sources and the voluminous secondary literature. He concentrates upon the negotiations among the leaders, national and to a much less extent local, who established and maintained the *Deutscher Reformverein*, limiting his remarks upon the strength and social composition of the *grossdeutsch* popular support to the estimates that these leaders gave.

I found the author's chronological political analysis inadequate for the subject. The discussion of constitutional proposals, even for the organization of the *Reformverein* itself, aside from those for the German Confederation, includes so many ideas and so many plans in such brief form—what Schäffle, Bernus, or Fröbel said in this letter and in that—that the author loses himself and the reader in the trees. He often elaborates to no manifest purpose upon the arguments over political procedure among the association's leaders; apparently he found the details

in the unpublished correspondence of persons like Lerchenfeld irresistible. The *Deutscher Reformverein* needed to be investigated, and although they offer no surprise, the author's conclusions about its failure are convincing: the association lived upon the expectation that Austria would develop an effective interest in reforming the German Confederation, and it collapsed with the exclusion of Austria from the German constitutional organization.

Students of the period of German unification must read Dr. Real's volume, but his subject calls for not merely a political but a constitutional and what the Germans term a "sociological analysis." Until a volume of this kind is written, we shall not be informed about the strength and weakness of the *Deutscher Reformverein* and the *grossdeutsch* movement.

University of California, Los Angeles

EUGENE N. ANDERSON

AUS NÄCHSTER NÄHE: LEBENSERINNERUNGEN 1884-1927. By *Arnold Brecht*. (Stuttgart: Deutsche Verlags-Anstalt. 1966. Pp. 526. DM 38.)

No chapter of German history has been so often misrepresented as the brief era of the Weimar Republic. Vilified by Hitler, it has been frequently misunderstood also by foreign observers who saw in it primarily the seedbed of National Socialism, but failed to recognize the positive forces at work in it. At last, some convincing witnesses have taken the stand to correct this false picture.

After Erich Eyck's *History of the Weimar Republic*, Toni Stolper's biography of Gustav Stolper, Count Harry Kessler's *Tagebücher*, and Theodor Heuss's *Erinnerungen 1905-1933*, comes the first part of Arnold Brecht's memoirs. A scholar-statesman of impeccable character and wide horizons, an author who has no personal ax to grind, tells here brilliantly what he observed in the Wilhelmstrasse. Americans know him as distinguished professor of political science at the New School for Social Research since 1933; they are familiar with the high caliber of his writing from earlier books such as *Prelude to Silence* (1944) and *Political Theory* (1959). After a colorful description of his carefree youth in Lübeck and of early experiences in government service, he concentrates on the years 1918-1927; a second volume, soon to follow, will carry the story to 1965. In many respects, Brecht's eloquent testimony is still more important than that of Eyck and Heuss because he was closer to the great decisions of that decade. Brecht's political role began when he was appointed Cabinet Secretary by Prince Max von Baden, the last Imperial Chancellor; he criticizes him for never having personally explained the necessity of abdication to William II. Thereafter, Brecht served under seven Chancellors of the Weimar Republic, holding the key position as head of the constitutional division in the Department of the Interior until he was suddenly dismissed by a German Nationalist minister. He knew President Friedrich Ebert intimately, and he admired his courage and grasp of political affairs. Brecht enjoyed the friendship of Walther Rathenau, whose scintillating personality he assesses with fairness and sympathy. He does full justice also to Gustav Stresemann and his turn to the democratic republic in 1923; he was "mit dem Herzen bei der Sache." His observations at close range confirm my own interpretation of Stresemann's chancellorship.

Brecht has put serious students of German history forever in his debt by this magnificent reconstruction of a stormy decade.

Trenton State College

FELIX E. HIRSCH

THE GERMANS AND THEIR MODERN HISTORY. By *Fritz Ernst*. Translated from the German by *Charles M. Prugh*. (New York: Columbia University Press. 1966. Pp. xii, 164. \$4.50.)

THIS volume presents a series of special lectures on recent German history that Professor Ernst gave at Heidelberg University in 1961–1962. A historian belonging to the “younger” generation (those who had experienced World War I only as children or youths), Ernst had been an active supporter of the university reform movement in the 1950’s, which aimed at revitalizing the German university to fit the needs of modern society. One of the objectives of the movement was to re-emphasize the educational mission of the university as compared with pure research, and, in view of the difficulty of many Germans to cope with their history after the cataclysmic events of 1945, scholars of German history and especially of *Zeitgeschichte* had an important contribution to make to this process which for many Germans involved a genuine “re-education.”

As rector of Heidelberg University in 1945, Ernst felt keenly the students’ need to hear a frank and balanced interpretation of the last fifty years of German history by one who had lived through that difficult period but who had remained free from involvement in the Nazi movement. The purpose of the lectures, then, was to show how the Germans had actually experienced the events and crises of the past half century and particularly to try to answer some of the questions students have today about the rise and the nature of National Socialism. In calling upon his audience to try to understand the German people’s reactions during that time, Ernst did not intend to offer a national apology but rather, as he said, “an apology of the older generation to a younger. . . .” Ernst subjected the second German Empire to a critical reappraisal. To re-create the atmosphere of those times he cited copiously from letters, diaries, and works of middle-class German writers, one of the more original chapters of the book being a short literary history of World War I. The author offered no facile answers, no oversimplified judgments, and some non-German historians may not agree with his somewhat generous opinions regarding the attitude of university professors and students during the Third Reich. But he also unequivocally asserted what many older Germans refused to admit: that “everyone knew, at least after 1938, that Jews as well as leftists were being persecuted in an inhuman manner. Anyone who denies this is lying.”

This book was of course intended for German readers, to break the “conspiracy of silence” that had prevailed for many years after 1945, especially among the older generation of Germans. But Ernst was particularly anxious to have these lectures made available to the English-speaking world. The translation is adequate, and the usefulness of the volume is enhanced by Ernst’s critical commentary, but is marred by several minor factual errors and misspellings in notes added by the editor.

American University

CARL G. ANTHON

ARMY, INDUSTRY, AND LABOR IN GERMANY, 1914-1918. By *Gerald D. Feldman*. (Princeton, N. J.: Princeton University Press. 1966. Pp. xvi, 572. \$12.50.)

THERE have been a number of recent studies on the German Army's military and political role in World War I, but one of its most important spheres of influence, the economic, has thus far been neglected. Professor Feldman's solid book fills an important need. It is based on extensive research in West German archives, supplemented by work in American libraries. Access to materials in East Germany was, unfortunately, denied, although the archives at Potsdam and Merseburg cooperated by answering questions and supplying copies of some important documents.

Like most "triangles," this is not a simple story, and a brief review can hardly convey its complexity. The first of the book's four parts deals with the initial efforts of the military, primarily the Prussian War Ministry, to cope with the many unforeseen economic problems resulting from twentieth-century warfare. Considering the conservative bent of the ministry and the novelty of these problems, its approach was surprisingly open minded. Especially on the most crucial issue—the conflicting interests of industry and labor—the ministry tended to side with labor's social and economic demands against the industrialists' unashamed greed for profits. This relatively enlightened, albeit improvised, policy gave way in 1916 to the far more highly organized but hardly more effective "gamble" of the Hindenburg Program. The genesis of this program and of its chief administrative agency, the War Office, is treated in the second part of the book. While the adoption of the program presented a victory for heavy industry, the passage of its corollary, the Auxiliary Service Bill, amounted to a triumph for organized labor. The conflicting interests of these groups condemned to failure the well-meaning administration of the War Office by General Wilhelm Groener, to which the third and longest section of the book is devoted. His main adversary was Colonel Max Bauer, Ludendorff's right-hand man and, far more than Ludendorff, the real villain of the book. Ludendorff's influence, as shown in the fourth and concluding section, was far less absolute and his "dictatorship" far more uncertain and vacillating than historians have hitherto assumed. It has been customary to see the many internal conflicts in wartime Germany as primarily between civilian and military authorities. But in the economic sphere, as Feldman shows, the army itself was deeply divided and hardly deserves any credit for purposeful leadership.

Given the large scope of his subject, Feldman's account is remarkably complete. One might have wished for more thorough coverage of secondary materials, and the use of English equivalents for German terms and titles is confusing. But these are minor shortcomings. Some apt illustrations from *Simplicissimus* help relieve the unavoidably factual tone of the book.

Yale University

HANS W. GATZKE

LE PUTSCH DE HITLER À MUNICH EN 1923. By *Georges Bonnin*. (Les Sables d'Olonne: Bonnin Éditeur. 1966. Pp. 230.)

ALTHOUGH this book is essentially a collection of documents, it is peculiar in that all the documents have been translated from their original German into French and,

furthermore, have been arranged so as to carry the burden of the author's argument. Perhaps because of the author's skill in arrangement, the book has a quality uncommon in such collections: it is fascinating to read. The core of the work is the section containing the stenographic reports of the closed sessions of the Hitler trial of 1924. These were discovered by M. Bonnin in the Alexandria collection in 1959 and are made public here for the first time. Particularly interesting is the deposition of General Lossow, which seems to indicate that the Bavarian government group under Kahr was cooperating rather closely with Seeckt in Berlin and stood prepared to support the establishment of the "Directory," which Seeckt was considering at the time.

The author contends that behind this willingness to cooperate with Berlin lay the deeper intention to rearm Germany for operations against France. Documents drawn from the Seeckt Papers in the Military Archives at Freiburg, the Federal Archives at Koblenz, and the State Archives in Munich are brought together to substantiate this argument. Despite the fact that some further evidence of German rearmament in violation of the Versailles Treaty is indicated, there does not seem to be here any firm demonstration of a well-formed conspiracy involving Seeckt and the Bavarian officials.

Though the major theme of the book is not convincingly proven, this work is nonetheless a valuable and handy source of new documentation on the events leading up to the Hitler *Putsch*. In addition to the Lossow deposition already mentioned, the book also contains Seisser's notes on his conversations with Seeckt, Haniel von Haimhausen's reports to Berlin on political developments in Bavaria, and good, though conflicting, descriptions of the march to Odeonsplatz. There is little about Hitler that is new, and, in fact, the future dictator is treated almost as a secondary figure being used by the real powers in German politics.

Purdue University

JAMES H. McRANDLE

GERMANIIA POD VLAST'IU FASHIZMA (1933-1939 GG.) [Germany under Fascism (1933-1939)]. By G. L. Rozanov. (Moscow: Izdatel'stvo "Mezhdunarodnye Otnosheniia." 1964. Pp. 517.)

REVOLIUTIONNIY KRIZIS 1923 G. V. GERMANII I GAMBURGSKOE VOSSTANIE [The Revolutionary Crisis of 1923 in Germany and the Hamburg Uprising]. By D. S. Davidovich. (Moscow: Izdatel'stvo Sotsial'no-ekonomicheskoi Literatury. 1963. Pp. 335.)

ROZANOV laboriously expounds the common Soviet thesis that German fascism was the product of conspiring German monopolists, imperialists, nationalists, and "revanchists" who hoped to find a way out of the economic crisis by rearmament for eventual aggression against the Soviet Union. In this conspiracy the German plotters also had the full support of the British "ruling circles" and partly also of American financiers and industrialists. Using respectable Western sources, Rozanov dwells at length on the financial aid given the Nazis by German industrialists. His charges against American interests are less well supported.

The account of the transfer of power to the Nazis is factual, straightforward, and generally free from distortion. So, too, is the rendition of Nazi foreign policy

to the outbreak of the war with Poland. Rozanov is fortunate in not being obliged to resort to distortion because the Soviet Union was not involved in the intrigues that preceded the transfer of power, and in foreign affairs it consistently advocated collective security from late 1933 to August 1939. The story of Nazi oppression within Germany is not overdone, though the scope and nature of Communist opposition after 1934, based mainly on Communist sources, are probably exaggerated. The author wholly ignores the intellectual sources of Nazism.

Possibly aware of the weakness shown by the German labor movement during the rise of Nazism and of the criticism of the divisive tactics of the Communist party, Rozanov severely condemns the "Rightist leaders" of the Social Democrats as well as Leftist deviationists such as the "Brandlerites," Ruth Fischer, and Heinz Neumann, but he is also mildly critical of the German Communist party, which mistakenly saw the Social Democrats as its main enemy when it should have concentrated on creating a united front against the Nazis. His heroes remain nevertheless Thälmann, Pieck, and Ulbricht. In his conclusion Rozanov chides the Western Powers for their failure to learn from history in reconstructing the German Federal Republic for future aggression against the Soviet Union, forgetting that the "reborn German militarism" knows the way west as well as east.

Rozanov's book is, by Soviet standards, fair, factual, and reasonable. The Western reader will not learn much from it, but the Soviet reader should find it instructive. It includes an excellent bibliography in Russian, German, and English, but no index.

Davidovich's work, unlike Rozanov's, is a product of the Stalinist period of historiography at its worst. Facts and interpretations are shamelessly distorted; name-calling abounds. The Russian reader will receive from this book a perverted view of what actually took place during the fateful year 1923, of the Comintern policy, and of the personalities involved. Davidovich magnifies inordinately the scope and significance of the Hamburg uprising of 1923, and his accounts of the roles played there by the Comintern and the Soviet Politburo are perversions of the true facts. Brandler, in particular, is condemned in unmeasured terms, but Maslow, Fischer, Thalheimer, Trotsky, Zinoviev, and Radek are only slightly less excoriated. Borkenau, Flechtheim, and Arthur Rosenberg are condemned as "renegades." There is a select but relatively extensive bibliography in Russian and German, but no index.

City University of New York

I. STONE

THE WAFFEN SS: HITLER'S ELITE GUARD AT WAR, 1939-1945. By George H. Stein. (Ithaca, N. Y.: Cornell University Press. 1966. Pp. xxxiv, 330. \$7.50.)

To penetrate the ambiguities and contradictions of National Socialist administrative practice and to write a clear and informative description of the evolution of any of Hitler's agencies require energy, critical judgment, and caution. Fortunately, Professor Stein is richly endowed with all three qualities. He has worked his way through mountains of doubtless not always absorbing docu-

ments in Washington, Alexandria, London, and Munich and has found much in them that was not known; he has weighed the findings of predecessors in the field like Reitlinger and Trevor-Roper judiciously and corrected them when necessary; and he has resisted the temptation to suggest that he has found answers to all of the problems posed by his theme. As a result, he has written a satisfying book, the most complete and objective history of Hitler's elite guard that has yet appeared, and one that is not likely to be superseded easily or quickly.

The *Waffen SS* originated in an essentially illegal act of Hitler during his first year in office, when he created a private praetorian guard (the *Leibstandarte SS*) which was independent of the SS leadership, the party, and the government of the *Reich*. This formation was subsequently supplemented by a number of "Political Purpose Squads" (to deal with political dissidents) and "Death's Head Detachments" (to run the concentration camps). Hitler did not originally intend to make these bodies very large, and certainly not to convert them, or allow them to be converted, into anything resembling a separate army. They grew into a formidable military force almost accidentally: first, because the coming of war in 1939 made it seem necessary to give the armed SS detachments enough front-line duty to assure them of the respect of the German people when they subsequently performed their prescribed internal security functions; second, because the unexpected prolongation of the conflict made it expedient to enlarge the *Waffen SS* (which had demonstrated fighting qualities in the first campaigns that surprised professional soldiers) and gradually to expand it, contrary to the *Führer's* earlier concept, into a fourth branch of the *Wehrmacht*.

The author describes this evolution, the administrative problems and conflicts it caused, and the military achievements of the *Waffen SS* that justified it authoritatively. He also provides his readers with a fascinating account of the increasingly mixed national composition of the *Waffen SS*—of the thirty-eight SS divisions in 1945, none was exclusively German, and at least nineteen consisted largely of foreign personnel—and an assessment of the performance of its various units, as well as a balanced analysis of the organization's culpability for atrocities committed during the war at the front and in occupied areas. An impressive contribution to the historical record, this book will doubtless also play a part in the continuing West German controversy about the criminality of the *Waffen SS*, which has been caused by attempts of former members to rehabilitate it.

Stanford University

GORDON A. CRAIG

HITLERS STRATEGIE: POLITIK UND KRIEGFÜHRUNG 1940–1941. By Andreas Hillgruber. (Frankfurt am Main: Bernard & Graefe Verlag für Wehrwesen. 1965. Pp. 715. DM 78.)

THIS is a magnificent work based on a thorough study of all source material and literature available in German, English, French, and Italian, and on translations from Russian and Japanese. The notes, conveniently placed at the bottom of the page, reveal a critical evaluation of the sources.

The primary focus of attention is on Hitler's strategy from the collapse of France in June 1940 to the opening of the campaign against the Soviet Union a

year later. An introductory chapter carries us quickly over the war from its outbreak; the penultimate chapter reviews its course until December 1941. By strategy Hillgruber means not the purely military concept but that zone where policy and the conduct of military operations unite. He notes, furthermore, a clear distinction in Hitler's thinking between the great-power (*Grossmacht*) position of Germany—the *Kontinentalimperium* to be achieved by the conquest of *Lebensraum* in the East—and the world-power (*Weltmacht*) position when Germany, dominating Europe, would build a great high-seas fleet to be served by bases rimming the Atlantic and establish a great colonial empire in Africa, making it economically independent even for tropical raw materials.

The author also devotes considerable attention to the explanation of the policies of the other Great Powers because it is only with a sound grasp of those factors to which Hitler reacted that it is possible fully to appreciate his strategy. Roosevelt in this period emerges as a very courageous and strong-minded leader; the Japanese were no more scrupulous toward Germany than Hitler toward them; Mussolini's abysmal failures are nicely delineated; and for lack of documentary evidence Stalin's thought is inferred from Russia's military and diplomatic actions. Although Hitler had a keen realization of Britain's imperial interests and maritime traditions, he was blind to its age-old interest in the balance of power on the Continent, a blindness that left him quite unprepared for Churchill's refusal to accept peace after the fall of France. Hitler floundered around with SEA LION, with the aerial attacks on England, with the submarine campaign. The problem was to force England to accept peace and yet not to destroy the British Empire, for its disintegration would bring no profit to Germany, only to other powers. America's growing involvement—and for this and for its potential Hitler received sound reporting—meant that time worked against him. For a while he toyed with the continental bloc project, something that interested Ribbentrop much more than Hitler, who had ceased to believe in it even before Molotov's visit to Berlin in November 1940.

Hitler's course in this crucial year, according to Hillgruber's interpretation, represents in part a series of decisions as immediate responses to the actions of other powers, but chiefly a rather constant and consistent approach toward his penultimate goal, the establishment of a great German imperium in the East as a result of the destruction of Soviet Russia and the extermination of its "Jewish-Bolshevik ruling classes." Hitler's view that the Russian campaign would be swift and sure was shared by the top military leaders in Germany, in great contrast to the period prior to the campaign against France. During the planning stage of BARBAROSSA they were fully initiated into the *Führer's* determination to destroy a good part of the Russian population in order to create space for the "master race."

Despite the difficult style, the annoying use of the phrase "Anglo-Saxon" powers to mean Great Britain and the United States, and a few other minor defects, Hillgruber's book is a splendid study.

Department of State

HOWARD M. SMYTH

MEMOIRS 1945-53. By Konrad Adenauer. Translated by Beate Rhum von Oppen.
(Chicago: Henry Regnery Company, 1966. Pp. 477. \$10.00.)

HERE is the first volume of the long-awaited memoirs of one of the modern era's greatest statesmen. Surely in terms of problems confronted and surmounted, Konrad Adenauer's solid accomplishments are more impressive than Bismarck's clever triumphs. Unfortunately the high drama of Adenauer's role in reshaping postwar Germany is told in such a relentlessly dreary fashion that one wonders if the austere author were not writing it all down as an act of penance.

Adenauer did not begin his remarkable career as a national statesman until he was in his seventies. Within a decade he had taught a suspicious Western world to trust his leadership of a new Germany; he had brought his crushed and confused people to an era of stability and prosperity unparalleled in German history; and he had transformed Germany's position from pariah to ally. This economic, political, and diplomatic *Wunder* was accomplished in part by Adenauer's great personal qualities of persistence, diplomatic skill, and unperturbable sense of purpose. But he is quick to admit that he was mightily helped by other forces.

He pays particular tribute to other great Western leaders who sought his goal of reconciliation and consolidation: Jean Monnet, Robert Schuman, Winston Churchill, and Harry Truman. Stalin's Russia also contributed heavily to Adenauer's success. The constant threat of Communist aggression—made acute by the clumsy attack on South Korea—gave Adenauer a splendid opportunity to profit from Western need of German power and to urge that Germany not become another Korea. By adroit negotiation Adenauer won an end to industrial dismemberment, strengthened Germany's economic and political position, achieved sovereignty, and gained partnership in the Western alliance.

But first he had to attain political power at home. After his unexpected victory over Schumacher, Adenauer faced a major political decision. Most of his party urged a "grand coalition" with the Socialists. Adenauer wanted no part of it. He wisely saw the need in a democracy for a strong opposition party, and he wanted the SPD to fill that function. Further, he recognized that Schumacher's narrow and fanatical truculence, particularly in the areas of economic planning and foreign policy, would have made cooperation impossible. *Der alte Fuchs* used all his cunning to win over his own party. He invited his colleagues to his home on August 21, 1949, and, on that memorable night used his knowledge of wine and his advantage as host to cajole, lecture, maneuver, and force the CDU to accept his program and to nominate him as their federal Chancellor. The *Bundestag* later confirmed the choice by a majority of one vote—Adenauer's own. Any other vote, he comments laconically, "would have been hypocrisy."

The memoirs concentrate on foreign policy and set forth methodically, doggedly, and with infinite detail the chronological narrative of the author's program. The detail is of course valuable, as are the lengthy quotations from previously unpublished letters and the careful descriptions given of interviews with a dozen figures who helped shape the course of German history from 1945 to 1953. But these memoirs tend merely to catalogue rather than to illumine the issues of the many diplomatic conferences. Descriptions of the people involved are sin-

gularly unrevealing—as wooden and lifeless as the author's prose. The most dramatic events of his life become drab and uninteresting in this telling. Throughout the book the author permits only hints of his own inner strength, complexity, and humanity. This great man's achievements deserve a far better literary memorial than he has provided in his important but tedious book. The translation is intelligent and accurate.

Williams College

ROBERT G. L. WAITE

THE PASSING OF THE HAPSBURG MONARCHY, 1914-1918. In two volumes. By *Arthur J. May*. (Philadelphia: University of Pennsylvania Press. 1966. Pp. 496; 497-864. \$18.00 the set.)

PROFESSOR May belongs to the steadily declining school of scholars who, apparently for reasons of euphony, deprive a dynasty that has lost its throne of the proper spelling of its name as well. Naturally the substitution of *p* for *b* in "Habsburg" can in no way detract from the value of this very impressive work. It has some outstanding merits and a number of mostly smaller shortcomings, some of which must be laid at the door of the editor of the manuscript.

As to the decidedly positive features, no other work in the large and steadily expanding literature on the dissolution of the Habsburg Empire has to such length and depth discussed World War I public opinion, and here, in particular, the attitude of the press in the *Entente* countries and the United States. Regrettably, the writings of Virginio Gayda, who influenced public opinion abroad against the monarchy possibly as much as Wickham Steed or Seton Watson, are not mentioned. May has also come very close to the solution of an almost insoluble problem: a proper balance in the narration of the course of events in the Viennese center and those in the political theaters of Budapest, Prague, Lwów, and Zagreb. His judgments as to the evaluation of specific events and personalities as well as to the complexity of causes responsible for the collapse are cautious, moderate, and mostly sound. Different views on particulars are possible, of course.

In Volume I, a discussion of Fischer's thesis set forth in *Der Griff nach der Weltmacht* would have been welcome as far as it pertains to Austria. The intensity of civil resistance among the Czechs is perhaps somewhat underrated, and Hungarian willingness to support Austria with food is rated too generously. Commonly it is held that Viktor Adler represented the Center of socialist thinking and Renner the Right Wing. May takes the opposite view. To dub the unfortunate last Emperor, Charles, a harlequin monarch seems a harsh judgment; the evaluation of Arthur Schnitzler as an Austrian writer of second rank, a strange one. Spitzmüller was never in charge of a caretaker government, and the heir apparent Archduke Francis Ferdinand, while greatly interested in federal reorganization of the monarchy, was, according to the documents deposited in his *Nachlass*, never committed to it, and at the time of his death rather reluctant to initiate sweeping change. As to the military collapse the curious interpretation of the armistice terms by the Italian high command and its consequences, which made hundreds of thousands of Austro-Hungarian soldiers prisoners of war, is not discussed.

There are minor factual errors, some of them pertaining to the intricacies of imperial etiquette. Foreign words are repeatedly misspelled, and quite a few personalities discussed in the text do not appear in the index. More disconcerting is the inadequacy of many references. Frequently the date of interesting literal quotations from foreign and domestic newspapers and magazines is not given; sometimes references are even more vague or altogether missing. In all these respects the first volume is superior to the second.

Naturally such shortcomings cannot destroy the genuine distinction of a largely original work of great merit that shows solid evidence of years of dedicated research. Yet their elimination in a hoped for second edition would further enhance its value.

Rutgers University

ROBERT A. KANN

DAS VÖLKERMANNIFEST KAISER KARLS VOM 16. OKTOBER 1918:
LETZTER VERSUCH ZUR RETTUNG DES HABSBURGERREICHES.
By *Helmut Rumpler*. [Österreich Archiv. Schriftenreihe des Instituts für
Österreichkunde.] (Munich: R. Oldenbourg Verlag. 1966. Pp. 96. DM 11.)

THIS monograph utilizes the papers of Max Hussarek and of Johann Andreas Eichhoff in unraveling the story of the origins of Emperor Charles's promise to turn Austria into a *Bundesstaat* when that state already was in an advanced stage of decomposition. The author concludes that Hussarek deliberately assumed responsibility for the initiation of the manifesto of October 16, 1918, because of his desire to shield the last Habsburg from criticism. Though Charles actually turned to the idea of an appeal to his peoples only on October 11, his subsequent behavior, as documented by Mr. Rumpler, reveals an adroitness in averting direct accountability and in keeping his advisers unaware of his next move.

Bulgaria's collapse and Wilson's refusal to bargain or compromise with the Central Powers were the considerations paramount in his mind. The Austro-Hungarian Army was eager for an armistice as early as October 11. Foreign and military pressures, not a sincere belief in a "state-autonomy" for his nationalities, induced the monarch to embark upon the diplomatic play of October 11-16. Though the southern Slav question had worried both Vienna and Budapest unceasingly in 1918, the manifesto actually aimed more at a solution of the Bohemian problem. Rumpler has decided that Hungary consequently should not bear the blame assigned its ministers since 1918 for the manifesto's failure. He also is convinced that the express recognition in the document of the integrity of the lands of the Hungarian crown derived from Charles's determination to uphold his coronation oath and not from Magyar hints or threats.

Valuable appendixes demonstrate the evolution of the manifesto and the contemporaneous, but suddenly dropped, discussions of constitutional reform. It would be foolish to assume that the author believes that the appeal ever had a chance of success. Rather, he offers expert detective work and interesting side lights on the Emperor, Hussarek, and Burián, the Minister of Foreign Affairs.

Washington and Lee University

WILLIAM A. JENKS

DIE SALZVERSORGUNG DES WALLIS 1500-1610: WIRTSCHAFT UND POLITIK. By *Alain Dubois*. (Winterthur: Verlag P. G. Keller. 1965. Pp. xii, 748.)

So strong and pervasive is the human taste for salt that the salt trade has often penetrated deeply into otherwise underdeveloped areas. And its unusual importance to the primitive economies of these areas has often made it an object of great political concern. One such area was the Swiss mountain canton of the Valais during the sixteenth and seventeenth centuries. M. Dubois appears to be prepared to devote a good part of his life to the study of its salt trade. This monograph presents his study of the history of that trade from the beginning of the sixteenth century, the earliest period for which reasonably complete records of it survive, to 1610, when the monopoly of the trade was taken over by local merchants. The study begins with a rather summary sketch of political, economic, and geographic background. Most of the work is devoted to an extremely detailed narrative account of the negotiations undertaken by governmental authorities in the Valais with the foreign merchants who contracted to supply salt and with the representatives of foreign governments whose taxes constituted the chief variable cost fixing the price of salt. The most general development this narrative describes is a shift from dependence on Mediterranean salt from France to salt from Italy after 1574, as the religious wars made supply from France uncertain and expensive. A final section of the study analyzes with considerable sophistication such economic factors in the salt trade as supply, demand, costs, competition, relation of salt prices to the general price level, and intimate connections between the salt trade and the crudely mercantilistic monetary policies of the Valais. The entire study is based primarily on exhaustive research in Swiss archival collections, principally in the Valais, also in Geneva and Bern, the latter containing a set of transcripts of important Parisian manuscripts. The author has not explored collections in Italy and southeastern France, although they, as he points out, might help to round out parts of his analysis.

Altogether, this book should prove indispensable to anyone interested in its narrow subject, and it may prove useful, chiefly for its methodological suggestions, to scholars with interests in related aspects of early modern economic history.

University of Wisconsin

ROBERT M. KINGDON

ZWINGLI AND THE ARTS. By *Charles Garside, Jr.* [Yale Historical Publications, Miscellany 83. Published under the direction of the Department of History.] (New Haven, Conn.: Yale University Press. 1966. Pp. xiv, 190. \$7.50.)

In this sensitive and highly cultivated study the author focuses upon Zwingli's understanding of the arts in terms of their applicability to, and value for, Christian worship. He does not undertake to abstract from the evidence an independent aesthetic, but limits his examination to music and the visual arts on the grounds that they are pre-eminently the arts of the liturgy, the only point of departure faithful to the sixteenth century. Three subjects are developed with

special skill and effectiveness: Zwingli's own early and lasting personal interest and accomplishments in music, his debt to Renaissance humanism, and his conviction that artistic material representations of the saints and of divinity are idolatrous.

Zwingli's debt to humanism was ambivalent, for on the one hand he learned from humanists such as Conrad Celtis, presumably, a new appreciation for music, while on the other hand he derived from the humanists a profound dissatisfaction with the musical practices in the churches and an Erasmian emphasis upon the spirit over the letter and over the material that presaged his eventual rejection of paintings and graven images. Zwingli's radical formal principle that only those liturgical practices were acceptable that could be derived *ex ipsis fontibus*, from the Scriptures, gave to the Reformed Church the peculiar stamp that distinguished it from Luther's or even Karlstadt's more permissive and inclusive forms of worship.

So intimately is Zwingli's presence bound up with the history of Zurich for twelve years that the narrative broadens out to include an account of the iconoclastic cleansing of the temples in that model city. An interesting epilogue relates the resurgence of music even in the Great Minster years after Zwingli's death, with the introduction of congregational singing on the widespread evangelical church pattern and finally even by the installation of an organ.

But music and the visual arts continued to prosper primarily in the nonecclesiastical, not to say secular, setting. It is regrettable that the author could not present so complete a picture of Zwingli's personal reaction to the visual arts as he does of his response to music, but perhaps the myopic third man of the Reformation was not very articulate on the subject. In this present ecumenical era in which liturgical forms, ecclesiastical arts and architecture, and the relation of religion and higher culture are being re-examined, the retrospective insight into the origins of certain specifically Reformed Church attitudes provided by this volume will prove most instructive. This book was well worth waiting for.

Stanford University

LEWIS W. SPITZ

ITALIAN HUMANISM: PHILOSOPHY AND CIVIC LIFE IN THE RENAISSANCE. By *Eugenio Garin*. Translated by *Peter Munz*. (New York: Harper and Row. 1965. Pp. xxiv, 227. \$6.50.)

In his preface to the last Italian editions of this fundamental work, Eugenio Garin stressed two points: the book had not been written to produce a synthesis of Italian Renaissance thought, but rather to draw attention to some of its neglected features; and, secondly, regardless of the great amount of work done in this field during the last twenty years (Garin himself subsequently contributed four or five other basic volumes), this is, with slight changes, the work first published in 1947.

It is a pity that this clear statement has been omitted in the "unabridged" English translation. Bearing the author's intentions in mind, a fair appraisal should point out that this book still is the only available effort to span the humanistic

philosophy of two centuries, but that, twenty years after its first publication, it should serve serious students as a mere starting point—“*primo avviamento*,” according to Garin—for the study of the problems ventilated here.

The most significant of them focus on the early *quattrocento*. Even before the composition of Garin's book it had been observed that humanism after 1400 saw the rise of some aggressive claims: the superiority of an active over a contemplative existence, the value of earthly goods for moral action, the psychological need of passions and emotions in real life. Garin discovered these motives in a far larger area of humanistic literature than had been known, and combined with his discoveries an equally fundamental discussion of a related factor: the rise of a no longer medieval mode of historical thinking, a feature already noted by some students, but not as yet sufficiently understood in its connection with the new “philology” of the humanists. Other chapters show that the emphasis on *vita activa*, *bona externa*, and *passiones*, as well as the new historical-mindedness, were still characteristic of sixteenth-century humanistic literature and affected Neoplatonism, Aristotelianism, and “realistic” trends during the late Renaissance.

There is, in Garin's canvass, a trace of a not fully resolved contradiction between his stress on passions, material goods, and civic life—Aristotelian tenets—and his inclination to evaluate the rise of *quattrocento* thought as a triumph of Christian, in particular Franciscan, attitudes over Scholastic Aristotelianism. In subsequent publications, he was to work out a more precise notion of a civic-minded type of Aristotelianism, representative of early *quattrocento* humanists. Similarly, he was to provide more positive, fresh appraisals of Neoplatonism, Renaissance magic, and Hermetic literature as well as of Toscanelli's and Leonardo's “realism.” A historian will in the early work miss clear indications that an exceptional place was occupied by Florence during the period analyzed in the longest chapter, “*La vita civile*.” No effort is made to trace the growth of certain ideas to the impact of “civic society,” the city-state, or any other specific environment, either in Florence or elsewhere. Rather, we find in this chapter discussions not only of Florentine citizen-humanists, but also of the new “philology” of the Valla and Poliziano, of the beginnings of humanistic Epicureanism, of humanists in Rome and at the *Curia* and even at the Neapolitan court—of everything Garin wants to tell us about the early *quattrocento*.

A work that makes so many new contributions loses in usefulness if it lacks all indications of its relationship to other interpretations. In the new English edition this is, unfortunately, the case, for the annotated bibliography with which all the Italian editions are equipped has disappeared. Coming on top of the omission of the author's prefatory statements, this makes the English edition a mutilated book. Since both suppressed sections are brief, one may perhaps express the hope that the publisher will add them in an appendix to a future paperback, especially since Mr. Munz's translation deserves to become available in a book fully independent of the Italian original. Munz's rendering of the not always simple Italian text into clear and readable English is, in fact, a feat that deserves gratitude, even though the text has its imperfections, as indicated by some translation mistakes and factual errors (for example, “Petrarch knew Cicero [!] and Plato

only by their reputation . . . , rather than directly"). But these are minor flaws when weighed against the fact that one of the most widely needed Italian works on the Renaissance has now become far more accessible.

Newberry Library and University of Chicago

HANS BARON

THE GOVERNMENT OF FLORENCE UNDER THE MEDICI (1434 TO 1494). By *Nicolai Rubinstein*. [Oxford-Warburg Studies.] (New York: Oxford University Press. 1966. Pp. xii, 336. \$12.80.)

EARLY Medicean government perfected (having partly inherited) an amazing system of electoral controls. Power was the aim; the way was through established constitutional procedure. Virtuosity and slyness abounded. This outstanding book studies those controls, their growth and refinement. Demagogic assemblies (*parlamenti*), plenipotentiary bodies (*balie*), semirigged councils for determining political eligibility, commissions that half manipulated the pouches from which names were drawn for the *signoria*, and controlled election to other key offices were the constitutional devices used to erode the republican constitution itself. Drawing exhaustively on certain archival collections, this work is for experts. It is the most painstaking study ever done of the electoral procedures of an Italian city-state. Other cities await such thorough treatment. Wherever faction ruled, procedures were elaborate and tricky.

The author stresses both the constitutionality of Medicean government and the strength of conciliar opposition to it. Though diseased, the republic thus seemed to endure. And Rubinstein's thesis will endure, viewed from the standpoint of constitutional history. The questions that arise are therefore necessarily political. Limitations of space preclude my raising larger economic and social questions.

The old legislative councils often resisted the introduction of Medicean controls and such resistance was in a tough Florentine tradition. Not the opposition, therefore, but the inevitable consent is what was remarkable. Why were the old councils ready to abdicate in ways whose fatality they perceived? How compelling was the climate of intimidation, the vote buying, the horse trading? If political power is yours and organized opposition is illegal, can you not reduce opposition in council and ram through legislation by corrupting, intimidating, enticing, or ostracizing men from office by means of the electoral devices in your control? Such questions invite imaginative political analysis. The author publishes a startling document proving that under the Medici balloting in council was often public. Councilors voted *a fave scoperte*, meaning that their votes could be seen. This illegal practice came in after 1434 and facilitated intimidation. Partisans of the regime, strategically distributed in council, could give the cue to voters and keep their eye on them. Again, if *balie* sometimes resisted Medicean leadership, we must recognize that all signories in their early stage relied on a united inner oligarchy. When this group quarreled, the budding *signore* was himself menaced; united under him, the regime usually got its way. For early Medicean government was a mixture of wile, constitutional procedure, legal pretense, and vio-

lence. Government lawyers could drive rich caravans through legal loopholes. Did Renaissance Italy ever produce a despot without claims to a legal title?

University of California, Los Angeles

LAURO MARTINES

GALILEO REAPPRAISED. Edited by *Carlo L. Golino*. [UCLA Center for Medieval and Renaissance Studies. Contributions, Number 2.] (Berkeley and Los Angeles: University of California Press. 1966. Pp. viii, 110. \$4.00.)

AMONG the many celebrations in honor of Galileo in 1965, this is one of the best. It is not the usual uncertain symposium of authorities going in several directions at once, or rehashing the well known, but a tight group of specialists who work together in analyzing several important facets of their hero. Unusually felicitous is Giorgio Spini's study on "The Rationale of Galileo's Religiousness," a subject usually avoided when it is not covered with treacle. As that unusual combination of a Florentine, a Protestant, and a student of New England Puritanism, Spini brings an astringent and unconventional point of view. He notes that it never was a matter of Catholic versus secularist; Galileo remained a well-adjusted pragmatist like the other patricians or *cittadini*. It was, rather, the hardening of the Church into an empty shell, then the authorities' abuse of power, that brought about the conflict, for Galileo still believed in the old faith with the old freedoms, and basically he showed himself first a natural philosopher in the sound tradition, then also a Florentine patriot who did not accept oppression even under fine-sounding words, and finally a profound believer at least of the *Theologia gloriae* if not so clearly of the *Theologia crucis*.

Dante Della Terza in "Galileo, Man of Letters" shows that Galileo in his criticism of Tasso's turgid mannerism represents the perfect and mature literary culture of his own times, but that his very resort to the vernacular and his discarding of literary rhetoric led to the creation of a scientific language correct but lacking the ornate overtones of style.

Justice thus being done to the humanistic aspect, so often forgotten, the three remaining pieces deal with science. Ernest A. Moody writes of Galileo's precursors and shows convincingly that when the newly established merits of the medieval Scholastics are taken into consideration, it still does not amount to the creation of the "New Science." Richard S. Westfall comes to grips with the great problem of force. Although Galileo continually used the word in many ways, Westfall feels that he still followed uncertain images. Finally, Lynn White, jr., dealing with technology, takes vigorous issue with Koyré's "Platonist" interpretation, which has recently gained such authority. I have myself insisted that it should be corrected into "Pythagorean-atomistic," for Galileo tried to avoid the strictly philosophical ideas of Plato, but White is more intransigent. He not only vindicates the late Leonardo Olschki's original position, but he refuses the idea that technology comes from pure science. He brilliantly shows the role of technology in the Middle Ages and in Galileo's own thought. This becomes all the more convincing as it is supported by Geymonat's recent work on Galileo's operational philosophy. But that last word itself is a recognition of all that is conveyed by that great old phrase "natural

philosophy." Let us not forget that Galileo himself said that he had spent "more months on philosophy than weeks on mathematics."

Massachusetts Institute of Technology

GIORGIO DE SANTILLANA

LE PORT FRANC D'ANCÔNE: CROISSANCE ET IMPASSE D'UN MILIEU MARCHAND AU XVIII^e SIÈCLE. By *Alberto Caracciolo*. [École Pratique des Hautes Études, VI^e Section. Centre de recherches historiques. Ports, routes, trafics, Volume XIX.] (Paris: S.E.V.P.E.N. 1965. Pp. 306.)

THIS work follows now-classic studies of Fernand Braudel, Ruggiero Romano, and associates at the *École Pratique des Hautes Études* on economic *conjonctures* in the Mediterranean since the sixteenth century. Caracciolo's volume carries the study of the Mediterranean economy into the eighteenth century. This was a period of rapid transformation in political and economic structures in the Mediterranean world as the economic center of gravity continued to shift northward to the Atlantic and as England replaced French and Ottoman influence and emerged as the dominant economy in the Mediterranean.

Caracciolo has traced the economic evolution of the port of Ancona on several levels—in its internal aspects, in its relations to the hinterland, and in the framework of the international economy—and over a long period—from the mid-seventeenth to the early nineteenth century.

Ancona had reached its apogee as a great commercial entrepôt between East and West in the sixteenth century. During the long secular depression of the seventeenth century, Caracciolo points out, the port had been relegated to a lost corner of the Adriatic. Ancona recovered in a rapid and remarkable spurt of development in the eighteenth century, following the concession of the free port by Pope Clement XII in 1732, as England penetrated the Adriatic. The port attained, by the middle of the eighteenth century, a level of commercial development equal to that of Venice, though far below that of Trieste, Genoa, Leghorn, and Marseilles, entering into vastly expanded commercial relations with England and Northern Europe, exchanging agricultural products and industrial raw materials—cereals, hemp, and sulphur—for northern manufactured products. The spurt, however, was abortive, having been based on the extremely fragile foundations of intensified commodity exchange with low income and demand elasticities, without the simultaneous development of an industrial base in the hinterland to sustain economic growth. The revival, as Caracciolo points out, was short lived for a number of other reasons—geographic, political, institutional, and technological—and was followed by a new cycle of depression during the Napoleonic decades, which continued well into the *Risorgimento*.

The author has assembled a most impressive mass of archival material to document his story. Many documents have been included in appendixes. Graphs and time series, based on ship movements and tonnages, anchor taxes, customs receipts, and consular reports, are clear and conclusive. There are few data, unfortunately, on annual commodity movements (quantities and volumes), or on price series of imports and exports that are vital for a study of the terms of trade of Ancona with Western Europe in the eighteenth century.

The author suggests that the historical experience of Ancona has special relevance for countries developing today. The point is well taken. He has shown, convincingly, that the attempt to emerge from traditional structures can stretch over centuries and that successful development depends on very complex variables, indeed.

Massachusetts Institute of Technology

IRA A. GLAZIER

MASSIMO D'AZEGLIO: AN ARTIST IN POLITICS, 1798-1866. By *Ronald Marshall*. (New York: Oxford University Press. 1966. Pp. viii, 328. \$8.00.)

THIS conventional full-length biography deals with a versatile Piedmontese aristocrat who helped mobilize anti-Austrian sentiment as a novelist and pamphleteer, fought in the campaign of 1848, served as Victor Emmanuel's Prime Minister from 1849 to 1852, and acted thereafter as elder statesman, going on occasional diplomatic missions and serving as governor of Milan in 1860. This summary of the politically significant phases of Azeglio's career does not adequately characterize the course of his life. As Marshall painstakingly shows, Azeglio's talents and energies were only intermittently engaged by political affairs. He worked with much more persistence and enthusiasm to achieve professional competence as a painter and devoted more attention to private and frivolous pursuits than he did to the art of politics or even to carrying out his official duties. Marshall has succeeded in conveying a well-rounded portrait of the "whole man" who played a part-time role as political actor. He has also clarified Azeglio's position vis-à-vis other *Risorgimento* leaders and depicts him as a patriotic constitutional monarchist, eager to liberate the peninsula from foreign dominance but pessimistic about unification; equally opposed to popular sovereignty and papal supremacy; regarding idealists, *Realpolitiker*, and indeed almost all professional politicians and publicists with distrust and disdain.

Despite Marshall's subtitle, it is not as an "artist in politics" but rather as an aristocrat who exemplified many of the values and attitudes associated with his status group (honesty, loyalty, paternalism, frivolity) that Azeglio's career appears historically significant. Unfortunately the author has not unified his narrative by exploiting this theme. Nor has he provided any viable alternative. He merely follows his subject from cradle to grave, content to let "the facts speak for themselves," offering few clues as to his criteria for selection. Much that seems trivial is expounded at length; issues worth exploring are slighted. Like his eye for significant detail, Marshall's analytical powers seem weak. Occasional ventures beyond description into appraisal cast a very dim light on the subject at hand. He has, moreover, written a dull book about an entertaining personage. His presentation, nonetheless, has the virtues of its defects. He has no polemical ax to grind; he is fond of his subject without indulging in hero worship; and, by utilizing much unpublished correspondence, he has produced a well-documented, seemingly reliable account upon which other historians may draw.

American University

ELIZABETH L. EISENSTEIN

GARIBALDI AND HIS ENEMIES: THE CLASH OF ARMS AND PERSONALITIES IN THE MAKING OF ITALY. By *Christopher Hibbert*. (Boston: Little, Brown and Company. 1966. Pp. xvi, 423. \$7.50.)

THIS is Christopher Hibbert's twelfth book and one likely to appeal to many readers. In twenty-five short and readable chapters grouped into three sections the author describes high and low points in the seventy-five years of Giuseppe Garibaldi's life: his childhood in a poor but warm seafaring household; the call of the sea; the appeal of conspiracy; the dozen or so years in Rio Grande unsuccessfully trying to shake off Brazilian control, and in Uruguay successfully resisting Argentine aggression; the return to Italy in 1848 and the great adventure of 1849 in Rome; the exile's hard struggle to make a living and his peregrinations across the oceans; the pioneering in rocky Caprera; his greatest adventure, in 1860 (the greatest of all since Napoleon's); the slow decline punctuated by bursts of military, political, and literary activity. Although well documented and, on the whole, accurate, the book is closer to the historical novel than to academic history because of its style. It is a work for young readers who will delight in the extraordinary adventures of the man his contemporaries called a hero, in the description of battles, in Boccaccioesque episodes. As well as being delighted, they will learn some history.

The value of this work as a useful contribution to history is small, though fourteen pages of sources and over seven hundred footnotes testify to the diligence of the researcher. Whatever its shortcomings, the book was worth writing because it is worth reading by those who otherwise would not be likely to spend much time in learning history. There are the usual unimportant but often irritating minor errors and unbalanced treatment of leading participants.

The liveliness, readability, and footnoting do not mean that anything has been added to what is known of the man, his deeds and misdeeds. Part of the charm of Hibbert's story is its timelessness: Garibaldi was a type, to be found in all ages in many countries—brave, idealistic, romantic, sincere, egocentric, ruled by emotions, simple, and straightforward. To see not the type but the particular individual one needs perspective, a familiarity with what stirred him, and with the scene in which he moved. One has to relive in the mind and in the written page the times as they were when the man lived. In this the book is somewhat lacking.

Smith College

M. SALVADORI

POLITICA E MAGISTRATURA (1848-1876). By *Mario D'Addio*. [L'Organizzazione dello stato. Collana di studi e testi nel centenario dell'unità, Number 3.] (Milan: Dott. A. Guiffre, Editore. 1966. Pp. xii, 966. L. 7,000.)

LIKE the other volumes in this distinguished series, this latest study is intended for advanced specialists in the history of the modern Italian state.

A close commentary and analysis of the subject of its title take up the first 245 pages and are followed by 186 separate documents, including relevant parts of the major constitutions, speeches, letters, and articles. The central matter concerns arguments on the independence of the judiciary and the irremovability of

judges. Just how to keep the courts out of politics appears to be a perennial and, by now, quite familiar problem, since appointed judges are given their appointments by the executive power—even if with the advice and consent of some legislative body—but are supposed to remain an independent third branch of government. The question of assisting or by-passing the regular courts through the establishment of extraordinary tribunals and commissions was as live a point in 1848 as it has been more recently, as was the matter of courts acting, in effect, as legislators when the regular legislature is unable or unwilling to pass urgently demanded laws. The universality of this fine study's theme is proclaimed in its opening words: "Liberty and justice are almost inseparable terms. . . ."

Northwestern University

GEORGE T. ROMANI

L'ORGANIZZAZIONE DELLO STATO TOTALITARIO. By *Alberto Aquarone*. [Saggi, Number 372. Studi e documenti del tempo fascista.] ([Turin:] Giulio Einaudi Editore. 1965. Pp. ix, 620. L. 6,000.)

AQUARONE's volume is the first to appear in the "History and Documents of Fascism," a projected series edited by the distinguished historian Rosario Romeo. Each volume will consist of a lengthy historical essay and a number of primary documents. If the remaining books are as successful as this one, the collection will be of fundamental importance for the study of Italy in the Fascist period.

The author's general purpose is to establish the main line of the organizational structure and the internal conflicts of the Italian Fascist state. Accordingly, he has emphasized two problems: relations between the National Fascist party and the state, and relations between the party and the state on the one hand and Fascist labor organizations on the other. It is commonly assumed that an essential characteristic of Fascist regimes is the absorption of the state in the party, but Aquarone demonstrates that the opposite was true with regard to Italian Fascism. By 1926 the state had won a decisive victory over the party. This is the fundamental meaning of many of the documents reproduced in this book, especially the successive statutes of the National Fascist party and the *leggi fascistissime* of 1925-1926. In short, Mussolini's political defeat of Roberto Farinacci in 1926 was an Italian equivalent of Hitler's purge of Ernst Röhm and the SA in 1934.

All this material once again points up the complex nature of Fascism as well as the imperative need to distinguish Fascism as a social movement from Fascism as a governmental regime. Aquarone succinctly indicates this distinction: There was "a considerable change in the social composition of Fascist ranks and cadres between 1924 and 1927. . . . Fascists of 'the first hour' were expelled in great numbers, that is those belonging to the less wealthy groups who were proponents—however illogical and hesitant—of a program of social reforms. Instead, the gates were opened to the middle and upper bourgeoisie, to the industrialists and the landowners."

A large proportion of the second part of this book consists of documents relating to the corporate state and the labor movement—the pacts of Palazzo Chigi and Palazzo Vidoni, the syndical law of April 1926, the Charter of Labor, the corporative law of February 1934, and many others. Aquarone's general view

of the corporate state is certainly correct: it "still left ample possibilities of maneuver to the traditional entrepreneurial classes, whereas the working classes were regimented with much more forceful resolution." This is not to say that Italian capitalism and Fascism are to be equated. The truth is that Fascism never had a "sociologically coherent base, a class, section or group that was totally identified with it." This fine study should serve as a starting point for a whole series of works on the concrete relations between a historically developing Italian Fascism and the various classes and groups of the Italian social structure.

Rutgers University

JOHN M. CAMMETT

THE BRUTAL FRIENDSHIP: MUSSOLINI, HITLER, AND THE FALL OF ITALIAN FASCISM, Part I. THE SIX HUNDRED DAYS OF MUSSOLINI, Part III of THE BRUTAL FRIENDSHIP: MUSSOLINI, HITLER, AND THE FALL OF ITALIAN FASCISM. By *F. W. Deakin*. [Anchor Books.] (Rev. ed.; New York: Doubleday and Company. 1966. Pp. xii, 546; xii, 348. \$1.95; \$1.45.)

THIS two-volume paperback publication of an outstanding study of the final years of Benito Mussolini and his Fascist regime makes available to a broader public the major work of a distinguished Oxford scholar and master, the warden of St. Antony's College. In this revised edition the author tells us that "certain documents and appendices have been omitted, translations corrected, and most footnotes deleted. The chapters dealing with the interlude of the Badoglio government in the original version have been replaced by a new Epilogue at the end of the first volume, and by an Introduction to the second. . . . The new edition also embodies comments and corrections which have reached the author since the first publication of this book. A contemporary historian has one particular advantage over his colleagues working in more sheltered fields of history: his work, which is both provisional and provoking, is exposed not only to reviewers, but to survivors. This present record of the fall of Italian Fascism and the fate of the Axis alliance has been subjected to this rewarding treatment."

I can add only one more paean to the volume of praise that greeted the original edition upon its publication. It demonstrates conclusively how false were the claims of totalitarian (or pseudo-totalitarian) regimes to either monolithic unity or efficiency. The author's final comments on Mussolini as a person and political leader are highly perceptive. My only regret is that he did not choose to make a comparable summary judgment of the Fascist system (or lack of it) as a whole.

University of Connecticut

NORMAN KOGAN

TEUTON AND SLAV: THE STRUGGLE FOR CENTRAL EUROPE. By *Hermann Schreiber*. Translated from the German by *James Cleugh*. (New York: Alfred A. Knopf. 1965. Pp. 392. \$8.95.)

THE subject of this book is worthy of prolonged and devoted effort by a professional historian. Dr. Schreiber's main interests have been journalistic and literary; his large purpose is to sketch the whole history of the relationships between the

Germanic and Slavic peoples in the hope of redressing the nationalistic prejudices of their respective historiographies. He is interested also in the continuity of policy that relates, for example, the kinds of conquest perpetrated by the Teutonic Knights and by Hitler.

Schreiber, however, has historical prejudices of his own. He does not like the Middle Ages; he thinks the Renaissance was "the only auspicious age of mankind"; he uses much racial and blood terminology, as when he says that north German settlers infuse "the spirit of the Hanseatic League into peasant veins."

Modern Europe is for the author the outcome of the dramatic confrontation of German and Slav after the cessation of the Slavic migrations westward. It was then that the Slavs in Eastern Europe "started European civilization afresh at the level of its beginnings a thousand years previously." Schreiber condemns the brutality of German expansion eastward and likewise the Church's failure to intervene.

It is good to have Schreiber describe the German expansion for what it really was. He could have used Helmold of Bosau and Henry of Livonia more effectively to support his main thesis. His continuation of the story of German colonization beyond the medieval period contains fascinating details little known to our general history books. Hitler is made into a new, mad, nationalistic Herman of Salza. The "preparatory work of a monolithic academic front" of specialists on the history of German colonization is well and convincingly related to the details of Hitler's *Ostpolitik*, resulting in "the bloodiest frenzy of slaughter ever recorded." The resistance to Hitler that culminated on July 20, 1944, was led by the "most renowned and capable branches of the German nobility east of the Elbe. . . . It was as though the implacability of the Old Prussian rebels had returned, seven hundred years after their great uprising." The Slavic *Drang nach Westen* at the close of World War II the author considers inevitable, and the Oder-Neisse Line as permanent.

Despite the author's prejudices, his willingness to be repetitive and discursive, his relapses into prolonged and frequent quotations, and, in one instance, into mere notes, and his indulgence in exaggerated generalizations, he is to be commended for taking up a big and vital theme of European history and for being willing to relate in an often persuasive manner the nasty aspects of a medieval colonial and imperialistic past in Central and Eastern Europe to its more recent and nastier counterpart.

University of Massachusetts

EDGAR N. JOHNSON

HET CALVINISME GEDURENDE ZIJN BLOEITIJD: ZIJN UITBREIDING EN CULTUURHISTORISCHE BETEKENIS. Volume III, POLEN, BOHEMEN, HONGARIJE EN ZEVENBURGEN. By A. A. van Schelven. (Amsterdam: Uitgeverij W. ten Have N.V. 1965. Pp. 167. Cloth 16.50 gl., paper 12.90 gl.)

PROFESSOR van Schelven of the Free University of Amsterdam was, when he died in 1954, in the midst of his proposed multivolumed study on Calvinism during its prime. The first volume, covering Switzerland and France, had appeared in 1943,

and the second, on Scotland, England, and North America, followed in 1951. He left behind manuscripts on Poland and Bohemia and some rough text on Hungary and Transylvania; these materials, compiled and edited by a former colleague, have now been published as a third and necessarily final volume.

Readers familiar with the two sound preceding books will find this one somewhat less satisfactory. The brief segment on Hungary and Transylvania is clearly unfinished, and Van Schelven would have been better served had it not been included. The more extensive sections on Poland and Bohemia have weaknesses that appear to be occasioned only in part by the circumstances of posthumous publication. Van Schelven, unable to read Polish and Czech, relied heavily on such standard works as those of Kot and Gindely. While he stresses the various late sixteenth- and early seventeenth-century Protestant crosscurrents in East Central Europe (differences between the Calvinists and the Lutherans, Bohemian Brethren, and Unitarians are effectively depicted), he concerns himself with Calvinism's relationship to dominant Catholicism only when he reaches developments of the magnitude of the Jesuit surge in Poland or 1618 in Bohemia.

Although he tends, when confronted with a welter of events, to simplify rather than clarify, credit is due Van Schelven for his skill in presenting the distinctive ideas of a particular movement or individual. Especially well portrayed are the views of the town Calvinists of Bohemia and the goals of Peter Skarga. Novel conclusions are lacking, but interpretations are moderate and quite sensible.

In sum, this book contains little for the serious student of the cultural and intellectual history of East Central Europe. On the other hand, students of the general history of early Calvinism may find it useful.

American Association of University Professors

JORDAN E. KURLAND

THE MILITARY BORDER IN CROATIA, 1740-1881: A STUDY OF AN IMPERIAL INSTITUTION. By *Gunther E. Rothenberg*. (Chicago: University of Chicago Press. 1966. Pp. 224. \$5.00.)

PROFESSOR Rothenberg's earlier work on the *Grenzer* dealt with an *ad hoc* institution that became uniquely successful in defending the long Ottoman frontier of the Habsburg Empire and in addition often functioned as the only effective means of imposing the authority of the central government at Vienna on these remote regions. The story he tells here is less clear cut. After the apparent high point achieved in 1739, the Turks were never again a serious threat to Austria. Thus the main function of the border regiments disappeared. Increasingly they were employed elsewhere, and before long the border was regarded by Vienna chiefly as a recruiting ground for light troops, tough but hard to discipline. The units remaining on duty on the border were mainly concerned with sealing off the monarchy from the threatened importation of bubonic plague from the Ottoman Empire. The use of the *Grenzer* on distant battlefields soon proved to be incompatible with the principal economic feature of the system, namely that each soldier in the regiments was given a section of land whose yield was meant to support him. Small-scale absentee farming, for obvious enough reasons, does not work. Moreover, the central government, until far too late, was unwilling to reach

an accommodation with the Orthodox clergy, which in a community consisting to a considerable extent of exiled Serbs was necessarily influential. In large part as a result of this, the *Grenzer* became increasingly susceptible to influences emanating from the motherland of Orthodoxy.

It is Rothenberg's merit that he recognizes that he is dealing with an institution becoming moribund and certainly in the nineteenth century no longer to be saved by administrative expedient, no matter how cleverly conceived. The Austrian staff officers who, just before and at the end of the First World War, were considering seriously schemes to reactivate the military border were pursuing chimeras. Further, he explodes convincingly the myth still current in some quarters in Austria that the *Grenzer* were distinguished to the end by an unshakable *Kaiserstreue*. The chief stricture to be made upon this book, applying equally to Rothenberg's earlier one, is that, written essentially from the purview of the military archives, it does not give a really illuminating picture of life on the military frontier. Repeated description of the types of uniforms worn by various units is no substitute for well-rounded social history. But, this reservation apart, this is a valuable study.

Colorado College

PAUL P. BERNARD

GOSPODARSTWO MAGNACKIE W WOJEWÓDZTWIE PODOLSKIM W DRUGIEJ POŁOWIE XVIII WIEKU [Magnates' Estates in the Podolia Province in the Second Half of the 18th Century]. By *Władysław A. Serczyk*. [Polska Akademia Nauk, Oddział w Krakowie, Prace Komisji Nauk Historycznych, Number 13.] (Cracow: Zakład Narodowy Imienia Ossolińskich, Wydawnictwo Polskiej Akademii Nauk. 1965. Pp. 168. Zł. 27.)

SERCZYK's study is an investigation into the economy of the great landed estates in the palatinate of Podolia, one of the Ukrainian provinces of the old Polish commonwealth, from about 1750 to 1793. The former date indicates the approximate ending of the period of resettlement of the area devastated by the wars of the preceding century; the latter date is that of the second partition of Poland, when the right bank Ukraine, including Podolia, passed under Russian domination.

Podolia was a classical land of latifundia, whose basic economic unit was a "manorial estate," with an average of three villages attached to it. The serfs divided their time and labor between manorial lands and their own plots. Despite primitive agricultural technology and prevalence of the wasteful three-field system, the fertile black earth brought forth considerable harvest surpluses.

The author's research focuses on the relation of the latifundian economy to the market. In this respect the position of Podolia was highly unfavorable: access to the Black Sea was barred by the Turks; overland trade with central Poland was prohibitively expensive; Podolia's traditional routes to the West had been cut off by the Austrian annexation of Galicia in 1772. There remained, however, one infallible means to provide the cash income desired by the landowner: the so-called *propinacja*, that is, the Polish nobleman's privilege to be the only producer and seller of liquor within the boundaries of his estate. The absentee landowner

found in this system the means to live in style in the capital or abroad and to satisfy his political ambition.

Serczyk's monograph is based on an exhaustive use of archival materials in Poland and the Ukraine: accounts, records, and business correspondence pertaining to the administration of several of Podolia's former latifundia. The book is a valuable contribution to the economic history of Eastern Europe. It is refreshingly free from Marxist clichés, except for the use of the term "feudalism," which is misleadingly applied to the latifundian and serfdom economy of prepartition Poland.

La Salle College

IVAN L. RUDNYTSKY

COMINTERN AND PEASANT IN EAST EUROPE, 1919-1930. By *George D. Jackson, Jr.* [East Central European Studies of Columbia University.] (New York: Columbia University Press. 1966. Pp. ix, 339. \$8.50.)

THIS attempt to describe and evaluate the effort of the Comintern to utilize the discontent of the peasant masses of Eastern Europe for revolutionary purposes is based primarily on English and Russian sources, but also employs works in German, French, Czech, Bulgarian, Polish, and Serbo-Croatian. The framework of analysis is broad and inclusive. Of the Eastern European countries, only Hungary, Albania, and Greece are not considered. For purposes of comparison, the Green International and its various peasant parties are introduced.

Mr. Jackson's thesis may be summarized as follows: The leaders of the Comintern were anxious in the 1920's to make revolutionary capital out of the discontent of the Eastern European peasantry. Among other things, they wished to broaden their base of domestic support in the Soviet struggle for succession with a successful revolution in Eastern Europe. But the tendency of the Eastern European parties was to move in the direction of legality and electoral participation, not toward revolutionary insurrection. The strictures and the admonitions of Moscow, in particular the gradual Bolshevization of the parties, usually ran counter to the common-sense requirements of the local situation and over the years cost the parties much, if not most, of their followings. The astonishing electoral success that the Communists and their front organizations achieved in some provinces was probably due as much to the prevailing harshness of economic circumstance as to the discontent of ethnic minorities.

Whether growing electoral strength would have satisfied the requirements of the Comintern seems to me an open question. On Jackson's own account the electoral success of peasant parties tended to make them coalitions of divergent and even antagonistic groupings, hardly amenable to any kind of foreign control. Whatever the respective weights of economic discontent and ethnic pressure, it was certainly true that, percentage-wise, the greatest Communist vote was obtained in minority areas and equally true that the Comintern policy of promoting ethnic self-determination made political sense for a revolutionary Russia confronted with the anti-Communist regimes of the sanitary cordon. The decline in Communist strength was assuredly as much due to the ebbing of the revolutionary tide of 1917

and to the repressive measures of the Eastern European governments as it was to the mistaken policies of the Comintern.

Jackson's book, an expanded version of his doctoral dissertation, is somewhat loosely organized; his choice of words, not always precise. He is often unable to differ with his colleagues without denigrating their intelligence. Nonetheless, this is a first work of considerable promise, bringing forward a wealth of significant and hitherto unknown detail and improving our general understanding of the role of the Comintern in the Eastern Europe of the 1920's.

Wayne State University

R. V. BURKS

THE STRUGGLE FOR CRETE, 20 MAY-1 JUNE 1941: A STORY OF LOST OPPORTUNITY. By I. McD. G. Stewart. (New York: Oxford University Press. 1966. Pp. xii, 518. \$11.20.)

Soon after the British evacuated Greece in World War II, the Germans, in May 1941, employed a new military technique, the use of airborne forces, to conquer Crete. Despite their lack of sea power, despite a courageous stand of British, Australians, New Zealanders, Greeks, and Cretans, the Germans gained control of the Mediterranean island in ten days and thus secured the southern flank of their impending invasion of Russia. How and why the British suffered defeat have been absorbingly presented by a medical officer who fought there. Dr. Stewart's exposition is historical writing at its finest—accurate, thorough, balanced, objective, and clear; it is also beautifully reasoned and strikingly literary. He has combined prodigious research and careful logic to produce the best narrative of the campaign, the splendid Allied official histories and German accounts notwithstanding. His thesis questions, without pettiness, the prescience and leadership of authorities in London (Churchill and the Chiefs of Staff), in Cairo (Wavell and Cunningham), and on Crete (Freyberg and many of his subordinates), all of whom failed to react with vigor and flexibility. Fatigued by their withdrawal from Greece, terrified by the *Luftwaffe*, confused by the invasion from the air, handicapped by ill-prepared defenses, insufficient weapons and equipment, inadequate air and naval support, and misunderstanding in higher quarters, the defenders of Crete were overwhelmed. They lost their opportunity to inflict a setback on the Germans and help destroy the myth of their invincibility, to provoke postponement of the German attack on Russia, to deny Hitler the southern Aegean, and to retain for themselves a base from which to bomb the Ploesti oil fields and targets in North Africa. How close they came was apparent in the opposite conclusions that both sides drew from the experience. Convinced that airborne operations were too costly, Hitler never again launched a large-scale attack of this sort; his opponents were persuaded to the contrary, and Allied airborne units later in the war contributed handsomely to victories, notably in Normandy.

The Struggle for Crete is model military history, not only for its description and appraisal of a significant and controversial campaign, but also for its insight into the human comedy.

Department of the Army

MARTIN BLUMENSON

ABSOLIUTIZM V ROSSII (XVII-XVIII VV.): SBORNIK STATEI K SEMI-DESIATIPATILETIU SO DNIA ROZHDENIIA I SOROKAPIATILETIU NAUCHNOI I PEDAGOGICHESKOI DEIATEL'NOSTI B. B. KAFENGAUZA [Absolutism in Russia (17th-18th Century): A Collection of Articles for the Seventy-fifth Anniversary of the Birth and Forty-fifth Anniversary of the Scholarly and Pedagogical Activity of B. B. Kafengauz]. (Moscow: Izdatel'stvo "Nauka." 1964. Pp. 517.)

THE works of B. B. Kafengauz on the eighteenth century are well known to all students of Russian history. It is not surprising that his colleagues and disciples have chosen to honor this distinguished historian with a *Festschrift*. This volume comprises a brief survey of Kafengauz' professional career and research, a bibliography of his writings, and fourteen articles by different contributors. Their unifying theme is the nature of absolutism that arose in Russia in the sixteenth, developed in the seventeenth, and reached its apogee in the eighteenth century. The articles deal with the following subjects: political preconditions of the genesis of Russian absolutism (A. A. Zimin), popular resistance movements in the seventeenth and eighteenth centuries (E. I. Indova, A. A. Preobrazhensky, Iu. A. Tikhonov), Zemski Sobor from 1547 to 1653 (L. V. Cherepnin), evolution of the *prikazy* in the seventeenth century (N. V. Ustiugov), *mestnichestvo* (S. O. Schmidt), growth of bureaucracy (N. F. Demidova), political surveillance apparatus (N. B. Golikova), financial policy (S. M. Troitsky), government legislation on cities (Iu. R. Klokman), government policy in the non-Russian areas (N. G. Apollova), ideas of absolutism in eighteenth-century legislation (N. I. Pavlenko), "enlightened despotism" in Russia (N. M. Druzhinin), social aspects of Catherine II's reform of provincial administration (M. P. Pavlova-Silvanskaia), and reflections of absolutism in the visual arts under Peter the Great (N. A. Baklanova).

Since it is impossible to do justice to all the contributions, I will mention those that appear most interesting to me. Schmidt's contribution is perhaps the most original. He points out that the patrimonial rank system, whose nature changed from the sixteenth to the seventeenth century, was not just an invention of the Russian aristocrats to curb the power of the tsar; it could also be and was used by the sovereign to tie the aristocracy to his service. Zimin argues that the main task before the Russian sovereigns of the sixteenth century was the destruction of "feudal fragmentation," that is, of independent institutions of the old republic of Novgorod, of the former appanage principalities, and of the Church. Troitsky gives a penetrating comparative analysis of the state budget and financial policy between 1679 and 1795, and, incidentally, challenges some of the conclusions reached by Miliukov early in this century. Druzhinin's article is full of discriminating remarks and is especially interesting on Catherine II's interpretation of Montesquieu's "intermediary powers," and Pavlova-Silvanskaia has delved into sources neglected before and has come up with a fresh interpretation of the origins and implementation of Catherine II's reform of provincial administration in 1775.

This brief discussion should indicate the interest this volume has for students of modern Russian history.

University of California, Los Angeles

ANDREW LOSSKY

GESCHICHTE DER BEHÖRDENORGANISATION RUSSLANDS VOM
PETER DEM GROSSEN BIS 1917. By *Erik Amburger*. [Studien zur Ge-
schichte Osteuropas, Number 10.] (Leiden: E. J. Brill. 1966. Pp. xxxii, 622.
86 gl.)

THE Russian historian has sadly lacked two types of reference tools readily available to his Western European and American colleagues: a comprehensive and reliable historical gazetteer of the major public institutions and a cumulative repertory of administrators and officials. Professor Amburger has now given his colleagues these two aids. It is a pioneer job done almost without the benefit of preparatory literature. A reference work's worth, fullness, and reliability can be tested only through long-continued use; it must suffice here to suggest its contents and scope.

The chronological limits encompass the so-called "St. Petersburg period" of Russian history, a time of cultural Westernization and of bureaucratization. As the brief introductory chapter makes clear, it meant the assimilation and adaptation of Western institutional models and goals, while preserving and even reinforcing the autocratic power of the tsar. This explains the limited effectiveness of these institutions and the need for constant reforming and adjusting of the administrative machine, which resulted in the system's becoming unwieldy and inefficient.

Primarily on the basis of legislative sources, Amburger gives full information on the establishment, nature, function, and transformations of practically every public institution and government office of the Russian Empire. He thus describes not only the central institutions and bureaus, but also their local offices; he sketches the legal status and organization of the social classes, of the non-Russian nationalities, and of the churches and sects. Military and naval institutions are also fully treated. Particularly valuable is the effort at a comprehensive description of the local administrative institutions of all major regions of the Empire. As the author confesses, it was not always possible to obtain exhaustive and reliable information here since the vagaries of local administrative practices and structures have not been consistently preserved in the accessible legislative sources.

One of the most important features of Amburger's compendium are the lists of the chiefs of the institutions described, and, in an appendix, we are given the names of all chiefs of diplomatic missions abroad (by countries) and a list of the educational and scientific institutions (with the names and dates of their directors or heads). The reference value of the book is enhanced by a set of comprehensive and useful indexes: all persons mentioned, names of institutions and titles of officials, and place names. There is also a comprehensive bibliography usefully arranged to parallel the organization of the book. Finally, in an excursus, Amburger makes some judicious and pertinent observations on the nature and composition of the "ruling élite" of Russia.

The fact that the book is written in German should not preclude its use even by those who do not handle the language easily, since the basic facts are clearly and succinctly stated and easily located with the help of the indexes, lists, tables, and references to the legislative sources.

Columbia University

MARC RAEFF

PLANS FOR POLITICAL REFORM IN IMPERIAL RUSSIA, 1730-1905. By Marc Raeff. [Russian Civilization Series.] (Englewood Cliffs, N. J.: Prentice-Hall. 1966. Pp. xi, 159. Cloth \$4.95, paper \$2.95.)

THIS volume in the "Russian Civilization Series" is a contribution to the school of thought that sees the history of the Russian Empire not simply in terms of the background of the 1917 Revolution but rather as an autonomous development of ideas and institutions that might have had any one of a number of outcomes after the First World War depending on the contingencies of political leadership.

Professor Raeff's introduction to these documents, which are limited to reforms of the central administration of the Empire, suggests that the principal needs of the Russian government after 1725 were for communication, coordination, and legal rationality. The ten documents that he has selected, from the "Conditions" that the Supreme Privy Council sought to impose on Empress Anne in 1730 to the manifesto establishing a state Duma in 1905, provide graphic illustration of the range of alternatives that were considered. The failure of these proposals to achieve their purpose is attributed by Raeff both to the resistance of the autocrats to any diminution of their authority and to the failure of Russian society to develop within itself the autonomous organizations—estates, local authorities, and economic and social interest groups—that in other societies have served as a basis for modernizing integration.

A broader view of the reform movement that included the economic and social developments after 1861 would have required a more complex interpretation, but within the limits that he set for himself the author has provided a welcome addition to the teaching materials pertaining to the Russian Empire that are available in English.

Princeton University

C. E. BLACK

VNESHNIAIA POLITIKA ROSSII XIX I NACHALA XX VEKA: DOKUMENTY ROSSIISKOGO MINISTERSTVA INOSTRANNYKH DEL [Russian Foreign Policy in the 19th and Early 20th Centuries: Documents of the Russian Ministry of Foreign Affairs]. First Series, 1801-1815 GG. [1801-1815]. Volume III, IANVAR' 1806 G.-IIUL' 1807 G. [January 1806-July 1807]; Volume IV, IIUL' 1807 G.-MART 1809 G. [July 1807-March 1809]. Edited by A. L. Narochitskii et al. [Ministerstvo Inostrannykh Del SSSR, Komissiiia po Isdaniu Diplomaticeskikh Dokumentov pri MID SSSR.] (Moscow: Izdatel'stvo Politicheskoi Literatury. 1963; 1965. Pp. 838; 781.)

THE third and fourth volumes of this Soviet collection of documents dealing with Imperial Russia's international relations make a worthy addition to a mag-

nificent series. Volume III, covering the period from the beginning of 1806 through the Treaty of Tilsit (July 1807), presents materials concerned with the war against Napoleon, the Third Coalition, Alexander I's struggle against the Turks, and relations with other states. Volume IV carries the predominant French theme through to the war with Sweden and Alexander's meeting with Napoleon at Erfurt. This volume also contains materials dealing with Russian policy toward the duchy of Warsaw, Russia's expansion into Georgia and Azerbaidzhan, difficulties with England and Austria caused by the alliance with Napoleon, Russia's active role in the Balkans, and the establishment of diplomatic relations with the United States. Of the slightly more than one thousand separate items included in the two volumes, approximately half are published in full for the first time.

The scholarly apparatus provided for the study of these documents is impressive. Detailed explanatory notes add over a hundred pages of fine print to each volume. Extensive name indexes present short biographic notes on each person mentioned, and there are similar indexes of the subjects and geographical names discussed in the documents. Finally, a bibliography of published works containing documents on Russian foreign policy is appended to each volume. The documents published for the first time are printed as they were written; thus, most of them are in French, a detail that should be appreciated by Western students of international affairs who do not use Russian. Russian translations are supplied in every case.

Besides adding much new detail, the highly selected materials of these volumes appear to cast some light from a new direction upon Russia's international relations. Because they are directly concerned with many other nations, they will also be of considerable value to students of Western diplomacy. As the Soviet editors intended, Western scholars must now submit these documents to painstaking examination, prepared, if the evidence requires it, to readjust their interpretations of men and events.

Michigan State University

ARTHUR E. ADAMS

ISTOCHNIKOVEDENIE ISTORII SSSR: PERIOD IMPERIALIZMA. KON-ETS XIX V.-1917 G. [Study of the Sources for the History of the USSR Period of Imperialism. End of the 19th Century to 1917]. By V. I. Strel'skii. (Moscow: Izdatel'stvo Sotsial'no-politicheskoi Literatury. 1962. Pp. 602.)

ACCORDING to the author, the inspiration for this book was the "definite" condemnation by the Twentieth Party Congress of the worship of the personality of Stalin, with its accompanying call for an end to the perversion of historical truth and of subjectivism and vulgarization of the historical process. Specifically, the party condemned the unscientific principles of arbitrary selection of documents for purposes of historical research and of skimming the surface without deeply penetrating the essence.

The author disclaims any intention of surveying the problem "in all its aspects and manifestations," limiting himself to a brief exposition of the general principles of the use of source materials, followed by more detailed examination of the seven types of source material available in Soviet archives. These he lists as:

works of the founders of Marxism-Leninism (Communist party materials); business records of institutions, organizations, and enterprises and their publications; socioeconomic statistics; legislation and other legal documents; memoir and epistolary literature; the periodical press; and artistic and scientific *monumenta*.

Inevitably, the author contrasts bourgeois and Soviet scholarship, condemning the former as "idealistic" and inclined to use source materials only in so far as they can be fitted into preconceived theories, whereas Soviet scholars assemble, and subject to rigid criticism, all the facts to be gleaned from the widest possible variety of sources before drawing any conclusions. In the process they draw on archaeological, ethnographic, linguistic, and folkloric materials, though for recent times the emphasis is properly on the study of written documents. In this connection they rely heavily on the related sciences of paleography, diplomatics, epigraphy, heraldry, metrology, and chronology.

Strel'skii is aware that bourgeois historians had developed such a critical apparatus of major importance. He pays tribute to "C. Seignobos & V. Langlois, *Introduction to the Study of History* [Russian edition], St. Petersburg, 1899" and shows great respect for the advances made by V. O. Kliuchevskii, but he feels constrained to warn that bourgeois methods of the study of sources need further elaboration, particularly in the direction of keeping class analysis in mind. Yet, true to the directives of the party congresses of the Khrushchev years, the author aims his most pointed shafts at scholarly malpractices of the Stalin era (for example, a collection of documents on *The General Strike in South Russia in 1903*, in which the compilers used the official, and falsified, police reports and ignored the secret report of the governor of Kiev to the governor general).

Lacking the imprimatur of the Academy of Sciences, the book bears only the endorsement of the Ministry of Higher and Secondary Education of the RSFSR as a textbook for use in universities. There is no index.

Brooklyn College

JESSE D. CLARKSON

CONTRIBUTIONS À L'HISTOIRE DU COMINTERN. Published under the direction of *Jacques Freymond*. [Publications de l'Institut Universitaire de Hautes Études Internationales, Number 45.] (Geneva: Librairie Droz. 1965. Pp. xxv, 265.)

WITHOUT access to Soviet archives, students of the Comintern must continue to rely primarily on the memoirs and letters of former activists to provide further insights into the inner workings of the organization and to illuminate the personalities who guided its destinies. Because of the often secretive nature of Comintern activities and the obscure origins of its agents, much of this information is fragmentary, highly colored, and difficult to verify.

This collection of nine pieces, inspired, collected, and edited by Boris Souvarine, clearly reveals the difficulties facing the historian. The most noteworthy contributions are B. Nicolaevskii's unpublished thirty-year-old interview with "Comrade Thomas," which provides some fascinating details on how the Comintern financed its activities in Western Europe in the 1920's; Marcel Body's honest memoir of the original French members of the Federation of Foreign Commu-

nist Groups in Russia who served either as technical specialists or as propagandists among French troops in Odessa; and Lucien Laurat's memoir-monograph on the origins of the Austrian Communist party, which describes Ruth Fischer's disastrous debut as a revolutionary organizer. Unfortunately, the remaining excerpts either have been published elsewhere (M. N. Roy on Borodin, several of Trotsky's letters, Ervin Sinko's Moscow Diary) and, with the exception of Roy, do not belong here, or offer nothing new (Balabanova on the origins of the Comintern and Branko Lazitch on the Comintern schools). Souvarine's letter-brochure to Trotsky is tangential to the book's main theme. J. Freymond supplies a sensible but brief historical summary and fails to provide any clear guide to the book's purpose. To be sure, one cannot ignore the series of factional feuds and personal vendettas carried on under Moscow's watchful eye which characterize much of Comintern history, but as a number of recent studies have shown, it is no less important to place these in the context of Europe's postwar social and economic dislocation. There is no evidence here of this having been done. Consequently, only specialists will want to make the effort to uncover the few nuggets buried in this collection.

University of Pennsylvania

ALFRED J. RIEBER

Near East

THE ISLAMIC LAW OF NATIONS: SHAYBĀNĪ'S SIYAR. Translated with an introduction, notes, and appendices by *Majid Khadduri*. (Baltimore: Johns Hopkins Press. 1966. Pp. xviii, 311. \$8.00.)

SHAIBANĪ (750–804), a student in the second generation of the great legist Abū Ḥanīfa, appears to have been the first Muslim jurist to accord systematic treatment to what today would be called international law. Of the two works that he devoted to this subject, only the shorter, *Kitāb al-Aṣl* [The Basic Book], has been preserved; the other, *Kitāb as-Siyar al-Kabīr*, has come down only through the detailed commentary provided by as-Sarakhsī. What the Muslim jurists called *siyar*, “the conduct of the state in its relationships with other communities,” was originally treated under the general heading of *jihād*, the individual and collective duty of the Muslims to transform the territory outside of Muslim control into a Muslim domain. It is the merit of the legal thinkers of Baghdad in the second half of the eighth century to have detached, not in theory but in actual treatment, the problems of international relations from those of community expansion by warlike or peaceful means.

Shaibānī's presentation does not proceed from axiom or theory, but deals with cases more or less likely to arise in the relations between Muslim and non-Muslim political entities. For the most part the book consists of hundreds of questions addressed to Abū Ḥanīfa and the master's answers. Together with the clipped style of early Arabic prose, this gives a lively, almost dramatic tone to the exposé which makes one forget the casuistic character of many of the questions.

To the advantage of the reader, Professor Khadduri has rearranged the texts at some points to make the sequence less haphazard. The principal problems

that Shaibānī tries to solve are the conduct of an army in enemy territory, the spoils of war, trade and other peaceful intercourse between the Territory of Islam and the Territory of War, peace treaties, safe-conduct granted a non-Muslim subject of a foreign ruler, and apostasy. The remaining chapters do not strictly speaking deal with international relations.

The Muslim lawyer in quest of a system of international relations was handicapped by his faith, which considered the state "as an instrument for achieving a doctrinal or an ultimate religious objective, the proselytization of mankind." The law of nations as a set of temporary regulations became mainly a law governing the conduct of war and the division of booty. The practitioners of the early Abbasid Age tried to recognize lasting territorial divisions among powers of different faiths and to solidify an international power system that theory would not permit them to stabilize by the admission of the concept of a permanent peace between believers and nonbelievers. Muslim law was primarily personal even though the exigencies of political reality compelled a certain admixture of the territorial principle. It is striking how small a part traditions of and from the Prophet and his companions play in Shaibānī's presentation. The Ḥanafī also attempts to divorce nonbelief from any automatic obligation to wage war by requiring previous provocation. A less peremptory approach to the obligation of *jihād* was to remain a characteristic index of the political realism of a legal thinker. It is, however, only since the sixteenth (and effectively since the nineteenth) century that the concept of peace rather than war as the natural relation between states of different denominations became accepted by the leading Muslim powers.

Khadduri has made accessible a significant text previously unavailable in print, and his introduction is a very creditable achievement indeed.

University of California, Los Angeles

G. E. VON GRUNEBaum

HISTORY OF EASTERN ARABIA, 1750-1800: THE RISE AND DEVELOPMENT OF BAHRAIN AND KUWAIT. By *Ahmad Mustafa Abu Hakima*. (Beirut: Khayats. 1965. Pp. xix, 213. \$6.00.)

THIS volume by a Palestinian who became acquainted with eastern Arabia through five years in the Kuwait Ministry of Education focuses on the emergence as a power of a group of related families of Bedouin origin from whom the present rulers of both Bahrain and Kuwait are descended. In piecing together and sorting out what can be recovered of early Utbi history from contemporary Arab chronicles, local oral tradition, East India Company records, and European travelers' accounts, Ahmad Abu Hakima has performed a service not only for the princely descendants of the Utūb but for scholarship as well. The contemporary emergence of Saudi power in central Arabia has been studied by a number of modern historians, both Arab and Occidental. The history of the Persian Gulf as a unit has been surveyed by Sir Arnold Wilson, but the many lacunae in these histories will only be filled by recourse to close study of local history such as Abu Hakima has performed.

In covering thoroughly Utbi political history during the second half of the

eighteenth century, Abu Hakima has demonstrated considerable critical acumen in his handling of variant accounts of Utbi origins and of the reasons for the emigration of the Al-Khalifa to Qatar and finally to Bahrain. It is regrettable that such skill in research should be nearly obscured by petty annoyances largely of a stylistic nature: sudden chronological deviations, indefinite pronominal antecedents, repetition, redundancy, and excessive organizational guidance to the reader. Careful editing would have helped the presentation immeasurably.

A more serious weakness lies in the author's treatment of the economic aspect of Utbi affairs. To characterize the wealth of the al-Hasa region in the eighteenth century as "fabulous" invites incredulity. The same skepticism attends his description of the Utbi portion of the Persian Gulf trade as "enormous," when Sir John Malcolm's figure of forty lacs of rupees as the measure of trade with India is shared by Abu Hakima's own testimony among Muscati, European, and Utbi vessels. He contradicts himself by saying that Kuwait "profited greatly from ships' cargoes" drawn by the transfer in 1793 of the British factory from Basra, when later he notes that "the gain from the British stay was largely political, not economic." To say, finally, that the Great Desert Route has never been seriously studied seems an injustice to Christina Phelps Grant's *The Syrian Desert*, when Abu Hakima's subsequent remarks on the mechanics of the caravan trade add little to her account. It would seem, in substance, that his perspective on the economic role of the Utüb is too narrow and his hand less sure than in the tracing of political events.

University of North Carolina

HERBERT L. BODMAN, JR.

SAUDI ARABIA IN THE NINETEENTH CENTURY. By R. Bayly Winder.
(New York: St. Martin's Press. 1965. Pp. xiv, 312. \$8.50.)

Sr. John Philby's *Sa'udi Arabia* has hitherto furnished the best history of nineteenth-century developments in the center of the Arabian Peninsula. Now he is superseded by Winder. The main lines and principal incidents of the story of the second Wahabi state are not altered, and Philby's work retains its value. But this book is twice as detailed as Philby's on the period from the Egyptian conquest of 1818 to the Rashidi triumph of 1891. Winder corrects errors of Philby and others. His focus is also somewhat wider: Nejd is still at the center, but Saudi relations with the Persian Gulf sheikdoms, with the British, and with the Ottomans are given greater consideration. There is doubtless information still to be gleaned from unpublished British or Ottoman archival sources, and perhaps new Arabic manuscript materials may come to light. It looks, however, as if Winder has canvassed the published materials thoroughly, and he has also exploited some important Arabic manuscript accounts. The thirteen-page bibliography, unfortunately unannotated, is valuable.

Prefaced by a succinct geographical and historical summary, the book starts in earnest with Mohammed Ali's victory over the first Saudi state in 1818. Winder examines the Egyptian occupation of Nejd, withdrawal to the Hejaz, reinvasion, and ensuing chaos (1818-1822); the Saudi restoration under Turki (1823-1834) and Faisal (1834-1838); and the renewed Egyptian attack, occupation, and

withdrawal (1836-1840). He then devotes nearly one-third of his book to the events of Faisal's second reign (1843-1865), the apogee of the second Saudi state, and to government, economy, and culture. The rest concerns the melancholy fratricidal strife among Saudi rivals, bringing on the decline of the state and allowing Ottoman conquest of Hasa and the extension of power by the ibn-Rashids of Hail that culminated in their complete victory (1865-1891).

This is a complex tale of wars, raids, revolts, and many-sided rivalries. It is not always easy to read and sometimes seems to share the qualities of Arab chronicles—pearls of truth strung one after another. This is in part owing to the nature of Winder's Arab sources and becomes evident by contrast with Western sources when he has opportunity to use them. He does, however, provide occasional pithy summaries and pungent observations. A number of quotations are left in French, untranslated, and Turkish names and terms are put into Arabic form. There are also illustrations, adequate maps, genealogies, and a thorough index. This is a work of sound and careful scholarship.

George Washington University

RODERIC H. DAVISON

ISLAMIC REFORM: THE POLITICAL AND LEGAL THEORIES OF MUḤAMMAD 'ABDUH AND RASHĪD RIḌĀ. By *Malcolm H. Kerr*. [Published under the auspices of the Near Eastern Center, University of California, Los Angeles.] (Berkeley and Los Angeles: University of California Press. 1966. Pp. vii, 249. \$6.95.)

Two of the leading modern theorists of Islamic reform—as contrasted with traditionalism and radicalism—were the Egyptian Muḥammad 'Abduh (1849-1905) and his Syrian disciple Rashīd Riḍā (1865-1935). Malcolm Kerr scrutinizes their reformulations (Riḍā's in particular) of classical Islamic political theory and jurisprudence, shows their weaknesses as intellectual reconstructions, and suggests that these largely explain their lack of influence on the politics and law of modern Muslim countries.

The classical theory of the caliphate was ordinarily content to state the divinely authorized ideals for the government of Muslims, without much concern for more practical questions of power and functions, such as effective means of deposing an unjust ruler. These questions became urgent in modern Muslim societies, with their many unsatisfied needs. Riḍā attempted to answer them with a new theory of the caliphate, but in a vacillating way that still left vague the respective roles of the caliph and the community.

Classical jurisprudence derived all laws from divine legislation, then tried to justify needed developments of law by methods of extending revelation, such as analogy, and even social utility. Kerr is particularly good at unraveling the delicate relations between this last and the original principle of "idealism," the divine source of all law, and shows how ingeniously the two were harmonized by medieval jurists. But the ferments of social and intellectual modernity cracked the old bottles and made both necessary and possible a new theory that would recognize "natural law" and social utility as rational sources of state law, independent of revelation. Both 'Abduh and Riḍā worked out the relations of revelation and

reason in this sphere, Ridā with some elaboration, but neither made clear or consistent the mutual limits of the two sources.

These are the leading ideas of a book rich in detail, stuffed with complex analyses and critical comment. Kerr applies concepts of modern political science to the intellectual world of Islam, with which he is thoroughly familiar through Arabic and Western writings. This is not an introductory work, but, after the more general studies of J. Schacht, A. Hourani, and others, it will prove highly illuminating.

University of Michigan

GEORGE F. HOURANI

Africa

NATIONAL UNITY AND REGIONALISM IN EIGHT AFRICAN STATES: NIGERIA; NIGER; THE CONGO; GABON; CENTRAL AFRICAN REPUBLIC; CHAD; UGANDA; ETHIOPIA. By *Richard L. Sklar et al.* Edited by *Gwendolen M. Carter*. (Ithaca, N. Y.: Cornell University Press. 1966. Pp. xiii, 565. \$10.00.)

THIS book represents the third and final volume of essays on individual African states edited by Gwendolen Carter. The first, *African One-Party States*, appeared in 1962, and *Five African States: Responses to Diversity* was issued in 1963. While a total of nineteen countries have been investigated by different authors within various disciplinary patterns, there has, nonetheless, been some attempt to achieve internal unity in each of the volumes by following basic formats so that the reader will not be totally confused. Furthermore, each of the articles in the series was designed to provide a broad coverage of historical, economic, and political information as well as a basic understanding of the sources of action and the distinctive features of the particular country under investigation.

In addition, the eight countries studied in the latest volume have the common problem of maintaining their unity in the face of potentially divisive internal and supranational regionalisms. Nigeria has its three troublesome provinces and antagonistic tribalisms. The Ethiopian Empire is confronted by a Somali nationalism that is irredentist in nature. The four states that at one time comprised French Equatorial Africa lack strong traditional political systems, have a sparse but heterogeneous population, and, individually, do not seem to have any unifying geographical, social, and economic forces; yet, they did share a common colonial experience, and certain ties have been retained from the days of French rule. Uganda is, of course, beset by the traditions of the several historical states found within its borders and dogged by the demands of state building. Niger, the least known of the components of the former Federation of French West Africa, suffers from its size, poverty, and its diverse population; it survives largely because its dismemberment would cause a crisis that would affect both West and North Africa. Like other African states, the eight countries discussed here share the mundane and macrocosmic problems of the underdeveloped world: lack of trained

personnel, shortage of capital, poor communications, monocrop economies, political instability, and insufficient educational facilities.

To a large degree, the eight articles were clearly designed to introduce students and general readers to the politics and problems of the African states under investigation. Two of the offerings, however, John Ballard's study of "Four Equatorial States" and Virginia Thompson's "Niger," deserve recommendation if for no other reason than they investigate areas about which little is available in English. I found that Donald Rothchild's and Michael Rogin's article on Uganda and Richard Sklar's and C. S. Whitaker's essay on Nigeria contributed little that was new in the way of approach and substance. Robert Hess's overview of Ethiopian history suffers from small but annoying mistakes and oversights, and one can easily argue with many of his assessments concerning historic and modern Ethiopia. The conclusion by Carter and Hess is reasonable but undistinguished, like the book itself.

Howard University

HAROLD G. MARCUS

HISTOIRE GÉNÉRALE DU MAGHREB: ALGÉRIE—MAROC—TUNISIE;
DES ORIGINES À NOS JOURS. By *Marcel Peyrouton*. Preface by *Jérôme Carcopino*. (Paris: Éditions Albin Michel. 1966. Pp. 284. 18 fr.).

A GENERAL history of the Maghrib written by an intelligent Frenchman with considerable experience as high-level administrator in all three former French sectors of the Arabic "West" could be cause for rejoicing both by specialists and general readers. Peyrouton, Vichy's last governor general in Algeria, relies basically upon the excellent secondary works available in French, such as those by Gsell, Carcopino, Monceaux, and Diehl, for his early chapters. Then he fills in, he tells us, from the various French archives and the work of scholars like Marçais, Julien, and Le Tourneau for the French epoch in Northwest Africa. This much he informs us in his preface; unfortunately, serious scholars will seek in vain the guidance of precise footnote documentation. Surely we would like to know which passages are based upon the French archival materials, and a bibliography or a bibliographical essay, plus an index, would render further service.

The text itself, well written and nicely proportioned to fit the survey nature of the book, is usually informative and lively. The chapter discussing Arabic art and thought between the eighth and twelfth century is thin, pedestrian, and insensitive. In the latter chapters of the work, when the various nationalisms become a force, again Peyrouton is the French administrator-patriot, not the historian. "Of all the old imperialist powers, France was the least imperialistic in the pejorative sense." In these late chapters readers could reasonably expect a few penetrating, insightful sentences out of the experience of this high-ranking administrator, but the author refuses to accept the challenge.

Taken as a whole the volume has value. Readers who already know the general volume in English edited by Nevill Barbour, and Le Tourneau's study of the 1920-1961 period, will not be greatly rewarded by this more recent survey.

Oakland University

RICHARD M. BRACE

THE PASSING OF FRENCH ALGERIA. By *David C. Gordon*. (New York: Oxford University Press. 1966. Pp. 265. \$6.75.)

THIS analysis of how Algeria fought for its identity and how, finally, France accepted Algeria's quest, well knowing that what France lost on the political battlefields it might more than counterbalance in the cultural zone, is insightful. Its greatest asset is that it gleans quite a few books published in France since Algerian independence, as well as periodicals, government handouts, plus a few documents, and assembles data from these, against the question of Algerian identity. Though the author disclaims that he has written a history—and he has not—still, his materials give us new knowledge of the revolution and the first three years of independence. Professor Gordon's study ends on July 19, 1965, the day Boumedienne replaced Ben Bella.

By now we know a little more about the inner workings of the Algerian side of the revolution as various members of the original leadership (Boudiaf, Aït Ahmed, and Khider), ousted by Ben Bella after independence, write books, give interviews, or do both. The miracle seems to be that, at least on the surface, factionalism did not entirely disrupt the FLN-ALN effort during the seven-year struggle against France. In retrospect, possibly the "kidnapping" of five of these leaders from the Air Maroc plane in 1956 worked a disadvantage on the French military and political effort to check the revolution. In any case, Gordon's account furnishes us with new understanding. We still do not know exactly how effective the Challe Plan for pacification really was. The Challe-Morice lines surely helped to tighten the ring, but the line was penetrated without undue casualties in 1961, as various eyewitnesses attest. The camel corps supply line, which Gordon does not mention, easily flanked the Challe-Morice western line and managed apparently to deliver quite a few weapons into southern Algeria.

That particular struggle is now finished, and the making of a new state becomes far more interesting and important. To the observer who watched the revolution and talked to the men, and who could not help admiring the courage, will, and organization of the Freedom Fighters, the first years of independence with all the division, error, and petty personal jealousy were, as Ronald Mathews noted, a barely supportable burden. On these points Gordon writes with considerable *expertise* and balanced judgment. He shows the terrible pressures on these people who stand rather in the midst of a cultural no man's land, trying to find out who they are and where they go. Clearly this is a return to Arabism, but at the same time the urgent need to produce, to train human resources, and to modernize is obvious to all the leaders. In this crisis De Gaulle has seen clearly and quite far. France's mission in Algeria and in the third world is enormous, given that country's resources.

This book, then, not only gives readers an intelligent up-to-date account of Algeria's search for identity; it also shows how De Gaulle has made decolonization an opportunity rather than a lament.

Oakland University

RICHARD M. BRACE

DREAM OF UNITY: PAN-AFRICANISM AND POLITICAL UNIFICATION IN WEST AFRICA. By *Claude E. Welch, Jr.* (Ithaca, N. Y.: Cornell University Press. 1966. Pp. xv, 396. \$8.00.)

THIS book is concerned primarily with the difficulties of establishing political links between African territories. Pan-Africanism is considered only in a peripheral manner. Realities of unification are measured against philosophies of unity expounded by African politicians using four examples: Togo, the Cameroun, Senegambia, and the Ghana-Guinea-Mali Union.

In each case Mr. Welch shows local issues being more important than general goals, as in the mistrust of the Gold Coast Anlo in Togo. The presence of boundaries and differential colonial regimes also had an inhibiting effect upon unification. Politicians in each area had to secure concessions within the framework of a particular system of rule, and these had to be compatible with ends defined by the imperial power. African parties were created that stressed devolution of power in a single state. Independence of their areas became the real goal of all major politicians. Other issues such as boundary rectification, tribal unification, or Pan-Africanism became subordinate to this need. The creation of national parties with dominant leaders in turn worked against cooperation between states after independence. In all areas surveyed by Welch, different European languages and cultures were deterrents to close association.

Colonial administrations were most important because they formed the attitudes of the elite and ultimately controlled the type and amount of power exercised by Africans. Welch shows that no real cooperation between French and British colonies had been effected. The desires of Africans for integration of differing territories had no precedent upon which to build.

The League of Nations in Togo and Cameroun exercised little control over Britain and France, who governed their mandates as they wished. The philosophy of control of the United Nations was different, yet it did little to further unification in Togo and Cameroun until Britain and France in their own empires had begun to grant political power to Africans. Even then the major UN role was in determining when trusteeship would end.

By selecting four complex areas to examine in a short book, the author imposed almost insuperable limits upon himself. The weakest sections concern the Senegambia and the Ghana-Guinea-Mali Union. The most original area of his investigations concerns Togo. Even here the work falls short of expectations. Welch has compiled much useful information from the all-Ewe movement to independence and unification, yet owing to space limitations he leaves many questions unanswered. This is, nevertheless, a valuable book, largely because of the wealth of factual material relative to territorial integration in West Africa.

San Jose State College

HARRY A. GALEY, JR.

SOUTHERN NIGERIA IN TRANSITION, 1885-1906: THEORY AND PRACTICE IN A COLONIAL PROTECTORATE. By *J. C. Anene.* (New York: Cambridge University Press. 1966. Pp. xii, 359. \$8.50.)

IMPORTANT and useful, this book is based on solid research and is clearly written.

The account of the imposition of British rule on southern Nigeria is complicated, but this is unavoidable given the proliferation of African authorities and confusions of imperial policy.

Professor Anene begins by revealing subtle political and cultural patterns. His subsequent narrative shows that British officials lacked the training and patience to discern such complexities. When plans miscarried, their stereotyped reaction was to blame some scapegoat, such as King Jaja of Opobo. Anene is impatient with the hypocrisy pervading the "Protectorate" issue. This was at its worst in the illegal and deceitful abduction of Jaja. He details the violence and intimidation that marked the extension of British rule and indicates where he takes a more critical view of imperial activity than earlier non-Nigerian historians.

The humanity of Salisbury and Consul General MacDonald is in marked contrast to their compatriots. Salisbury's sense of decency was affronted by the ruthlessness of his subordinates, but he failed to exercise effective control. MacDonald's patience and tact were reflected in his pacific methods. Yet, in 1893, he selected as his successor Ralph Moor, an advocate of a "forward policy" spearheaded by punitive expeditions, which epitomized British imperialism at this time. Investigation of Moor's career and motivation would provide a fascinating study of an imperialist in action. He was ruthlessly impatient in his political dealings with Africans while fighting skillfully to frustrate persistent attempts by British businessmen to jeopardize African economic welfare. If the "trade motive" was paramount, it had to be fair trade. Unfortunately, Anene does not attempt to resolve the paradoxes of Moor's paternalistic record. Anene considers that the British completely failed to understand the organization of the big Ibo and Ibibio groups, assuming their political life was anarchic and resorting to such disastrous expedients as the appointment of "warrant chiefs."

Anene supplements his documentary sources by reporting some valuable results of research into oral tradition by Ibadan students. He also includes some interesting old photographs. The maps are barely adequate.

University of York

HENRY S. WILSON

KINGDOMS OF THE SAVANNA. By *Jan Vansina*. (Madison: University of Wisconsin Press. 1966. Pp. ix, 364. \$6.75.)

THIS signal contribution to African history and to the writing of history more generally has emerged from the scholarship of one who is ranked by many as among the foremost of the contemporary historians of sub-Saharan Africa. It reports in such detail as is presently possible essentially the political histories of the major states of Central Africa (Congo, Angola, Zambia), the Kongo, Luba, Lunda, Kazembe, and Lozi, of the "minor peoples" influenced by them, and of the colony of Angola from approximately the early sixteenth to the early twentieth century. No other historian to my knowledge has attempted quite this task, and no one of any discipline has succeeded in providing such historical depth or synthesizing so much material about this relatively superficially known region of the continent.

Synthesis, in the sense of thematic interpretation, is kept to a minimum

because of well-recognized gaps in the data. Vansina, nonetheless, summarizes certain leitmotifs that may be found in a comparison of the structural similarities of the kingdoms and their common experience of trade, especially the slave trade, both within Africa and with the non-African world. Among these are the dominant influence of the personalities of strong rulers, the possibilities of civil strife, involving outside powers, over succession to the throne, and the establishment of exploited, tributary provinces, systems of indirect rule, and the subsequent dissolution of the state. Each of these themes is familiar to non-Africanist historians, of course; the more significant contribution of the book, therefore, is the demonstration of their relevance to interpretation of these phases of Central African history. This in itself is a matter of some moment because African histories have so largely been written and read as the record of Europeans in Africa or of their impact upon generally "historyless people." Any work that presents the evidence for a history of Africans in Africa is greatly to be welcomed.

Vansina's previously notable, if still somewhat controversial, contribution to the field of history has been the development of the tools for research into oral traditions. At least in Africanist circles, his methodology has won considerable respect from both younger and mature scholars because it provides the means for uncovering and utilizing unwritten data from nonliterate peoples and of relating them to the kinds of materials essential to anthropologists, political scientists, and all other students of social change. This book is far more than a mere demonstration of his method, however, for oral traditions and unwritten linguistic and anthropological data are joined with a surprisingly vast amount of written documentation. The result, modestly though perhaps properly referred to as "a workbook" in political history, is to be commended highly to all Africanist historians for its substantive materials, to other historians for its illumination of a largely ignored aspect of human history, and to both for its revelation of the potential value of a complex research methodology.

Johns Hopkins University

ROBERT A. LYSTAD

AUX ORIGINES DE L'ÉTAT INDÉPENDANT DU CONGO: DOCUMENTS TIRÉS D'ARCHIVES AMÉRICAINES. By *François Bontinck*. [Publications de l'Université Lovanium de Léopoldville, Number 15.] (Louvain: Éditions Nauwelaerts. 1966. Pp. xiv, 479. 680 fr. B.)

SOME work that assesses America's role in African history is being done in this country. Here attention is devoted to its part in the formation of *l'État Indépendant du Congo*. Professor Bontinck presents nearly four hundred documents or excerpts that demonstrate mainly the role of Henry S. Sanford, entrepreneur and former US minister to Belgium, in the foundation and formative years of Leopold II's Congo. The title is somewhat misleading; the book is actually a history, with extensive textual material accompanying the documents. The material (about half in English, the rest in French) comes largely from the Sanford Papers. Much of it was published over thirty-five years ago, but the editor has added new material.

Particularly interesting are the papers dealing with Sanford's role in persuad-

ing President Arthur and a reluctant Senate to recognize the flag of *l'Association Internationale Africaine* (AIA) as that of a friendly government, although it had no basis in international law. This was the first such recognition, and it was a crucial factor in the convening of the Berlin Conference of 1884-1885. Bontinck goes on to document the roles that Sanford and H. M. Stanley played at that conference. He then lapses into a chapter entitled "Stanleyana," in which he presents documents on a variety of subjects; some of them are relevant, but many are not. He concludes with a chapter and appendixes documenting several trips, both official and private, by Americans to the Congo during the next two decades. Of most consequence was that of G. W. Williams, the Negro historian, resulting in 1890 in one of the earliest published accounts of conditions there under Leopold II.

The material is well indexed, and the footnoting is both extensive and meticulous. Included are a wide and often illuminating body of correspondence between Sanford and Stanley and several exchanges with (Sir) William Mackinnon. This said, Bontinck could well have dwelt far more on Sanford's financial stake in a Congo controlled by Leopold and on the source and nature of his power in Washington, and he could have discussed more plausible reasons for his success in having the AIA recognized than his drawing a (patently absurd) parallel to America's experience with Liberia, and fancy dinners. Finally, he should have been far more ruthless in the editing. The important documents are all too often hidden beneath masses of material which is irrelevant and frequently trivial.

Ohio University

ALAN R. BOOTH

KING LEOPOLD'S LEGACY: THE CONGO UNDER BELGIAN RULE, 1908-1960. By Roger Anstey. [Issued under the auspices of the Institute of Race Relations, London.] (New York: Oxford University Press. 1966. Pp. xiv, 293. \$7.20.)

PROFESSOR Anstey has undertaken an important assignment in tackling the colonial history of the Congo. Although he has been preceded by two French scholars, a Marxist-Leninist (Michel Merlier, *Le Congo de la colonisation belge à l'indépendance*) and a frankly pro-Belgian colonial administrator-historian (Robert Cornevin, *Histoire du Congo-Léopold*), the need for a careful, thorough, objective assessment of King Leopold's legacy remained. Anstey's excellent earlier work on imperial diplomacy involving the Congo (*Britain and the Congo in the Nineteenth Century*) would provide hope that the present volume would meet this need.

Unhappily, the work is a great disappointment. It is structurally disjointed, contains incomprehensible gaps in coverage, relies far too heavily on a limited number of secondary sources, and in general fails to add much that is new.

Most puzzling for a historian of Anstey's demonstrated talents is the failure to cover large categories of source material. The author seems to have assumed that the fifty-year rule on government archives means that utilization of primary sources is precluded for students of contemporary history. His bibliographic note claims that "the non-availability of official documentary evidence for all save the

first decade of Belgian rule" necessitated "primary reliance on monographs, both books and articles." And yet the former Colonial Library in Brussels bulges with official records not covered by the restriction of secrecy: annual reports, proceedings of the *Conseil Colonial*, the *Conseil de Gouvernement* and provincial equivalents, official gazettes, debates of colonial advisory bodies, to mention but a few. He has consulted neither the Belgian nor the colonial press, although the latter in particular would have been an important source, especially for tracing conflicts within the colonial establishment. There is not a single citation from the important semiofficial review *Congo*, although this was an authoritative forum for policy debates from 1920 to 1939. Anstey has also devoted little attention to the vast body of anthropological literature. This inevitably makes his chapters on African societies in the Congo singularly inadequate, especially since "contact of cultures is the leading theme." It is staggering to see C. S. Seligman's partially discredited *Races of Africa* invoked as a source on the Bakuba, with Jan Vansina's many distinguished contributions on this and other aspects of Congolese history and anthropology nowhere cited.

It is pointless to extend the list of objections. The author did make fruitful visits to Belgium and the Congo, which armed him with some useful direct observations. The book is written in a style that would have done justice to better material: judgments made on Belgian colonial administration are on the whole fair and judicious, and there are some interesting and original insights into contemporary perspectives on the "red rubber" period in the Bolobo area. These merits cannot, however, compensate for the shortcomings of this prematurely published study.

University of Wisconsin

CRAWFORD YOUNG

THE ZULU AFTERMATH: A NINETEENTH-CENTURY REVOLUTION IN BANTU AFRICA. By J. D. Omer-Cooper. [Ibadan History Series.] (Evanston, Ill.: Northwestern University Press. 1966. Pp. xiv, 208. \$5.95.)

PROFESSOR Omer-Cooper has set as his purpose to analyze the *Mfecane* (the Nguni word used for the wars and disturbances that accompanied the rise of the Zulu) and "to relate the great chain of wars and migrations to the underlying processes of socio-political change and to attempt an assessment of the significance of the *Mfecane* in the history of Southern and Central Africa, the way it affected later developments and its enduring importance for an understanding of the contemporary situation." This is an enormously ambitious objective. Regrettably, it is only superficially achieved.

The Zulu, the Swazi, the empire of Gaza, the Ngoni migrations, Moshesh and the Basuto, Sebetwane and the Kololo, Mzilikazi and his Ndebele, and even the impact of the *Mfecane* on the southern Bantu are described in separate, successive chapters. Each is an efficient précis of what happened to the principal participants during the *Mfecane*, and although the pedestrian pace of the author's prose needs to be relieved occasionally by a flash of style, to have so luminously condensed the incredible complexities of the *Mfecane* is no small achievement. Indeed, present and future African historians without a specialized knowledge of Southern Africa

will undoubtedly bless Omer-Cooper as they comfortably crib their lecture notes from his finely chiseled summaries. But a summary is no substitute for analysis and seldom even for interpretation. What happens during the *Mfecane* is made wonderfully clear; the reasons why are not. Thus one expectantly turns to the final chapter, "The Mfecane in the History of South and East-Central Africa," only to find a disappointingly brief analysis of "the essential features of this revolution . . . [which] were the adaptation of the initiation ceremonies to military purposes and the creation of a permanent standing army organized in age-regiments." The author has made this point before. It is neither new nor startling in South African historiography. But why did this organization evolve as it did? Were not other elements (Zulu civil administration, for instance) involved? Why did not the revolution take different forms? Presumably, the answers are obvious to the author—simply "the reaction to conditions of frequent and severe warfare . . . stimulated by contact with the rudimentary age-regiment system of the Sotho peoples." Even with the brief elaboration of this theme that follows, such a response is as superficial as it is unsatisfactory.

Perhaps if the author had fixed more modest goals, such criticism would be unnecessary if not impertinent. Certainly his readers would have been quite satisfied with his unraveling the *Mfecane* without being led to expect more than they get.

University of California, Santa Barbara

ROBERT O. COLLINS

A HISTORY OF SOUTHERN RHODESIA: EARLY DAYS TO 1934. By L. H. Gann. (London: Chatto and Windus; New York: Humanities Press. 1965. Pp. x, 354. \$10.00.)

LEWIS Gann has written, for the second volume of his series on Zambian and Rhodesian history, a conservative, solid, straightforward, detailed account of the European presence between the Zambezi and the Limpopo. (For a review of the first volume, *A History of Northern Rhodesia* [1964], see *AHR*, LXX [Apr. 1965], 881; a third volume will carry the Southern Rhodesian story to 1953; and a related volume, though not one in this series, *Huggins of Rhodesia* [1964], by this author and M. Gelfand, was reviewed in the *AHR*, LXXI [Jan. 1966], 637.) The author has drawn extensively from the secondary literature and has thoroughly mined the Rhodesian Public Archives, for which he was editor, including British South Africa Company papers and a considerable variety of private papers—some from notable participants, a number from "ordinary" pioneers and settlers. Thirty- and fifty-year rules applied, and the obstacle is necessarily noticeable in the presentation of research in a form so definitely documentary-narrative as this. But the dispassionate, exhaustive kind of presentation, though it will not immediately compete with more dramatic and sweeping interpretations of Rhodesian problems, will in the long run be a doubly valuable counterweight to such ephemera. Not many countries are so fortunate so soon in their historiography.

In an obviously carefully planned balance of three nearly equal parts, Gann first treats early Portuguese and South African white penetrations culminating in

Cecil Rhodes's Chartered Company and "Pioneer" settlement in 1890 (but he cautiously—and wisely, in my opinion—eschews much comment on pre-European movements and settlements). In the middle third of the book, company rule and the emergence of self-government are traced; the shorter concluding section, when archival "closed" rules operate, deals with the opening years of Colonial Office rule after 1923. (In fairness, it should be added that the author would apparently prefer us to think of Rhodesia, as most settlers do, as a reasonably mature and self-sustained white society after 1902, when the Boer War and Rhodes's own life both came to a close, and this, indeed, is then the largest section of the book.)

Perhaps it is a happy chance that brought this volume—and this much of researched white Rhodesian history—to press before the Unilateral Declaration of Independence in November 1965. Even so, the concluding sentence of this volume cannot avoid the pertinent perception that "European rule itself called forth a dawning African race and class consciousness, and this loomed forth as a greater challenge to white Rhodesians than any faced by the Pioneers." This is history with meaning and relation, but with enough research and enough perspective to avoid the dangerous temptations of commentary on the current crisis.

Temple University

DONALD L. WIEDNER

THE CONSTITUTIONAL HISTORY AND LAW OF SOUTHERN RHODESIA, 1888-1965: WITH SPECIAL REFERENCE TO IMPERIAL CONTROL. By *Claire Palley*. (New York: Oxford University Press. 1966. Pp. xxv, 872. \$23.55.)

No short review can do justice to the exceptional merit of this book. Crammed to the last footnote with relevant detail, it lays bare the constitutional evolution of Rhodesia from its beginnings in 1889-1890 as a sphere of influence to its absorption within and later emergence from the abortive Federation of Rhodesia and Nyasaland. The second half of the book contains a painstaking examination of the 1961 constitution of Southern Rhodesia. An interesting addendum discusses the legal position respecting the declaration of independence in 1965 by Rhodesia's ruling cabal.

Throughout the vast length of her book the author dispassionately analyzes the practical as well as the legal consequences of constitutional forms and their development in Rhodesia. She demonstrates conclusively that the British government, both before and after the creation of Southern Rhodesia as a colony—misleadingly styled "self-governing"—in 1923, intervened vigorously and often in the affairs of the territory. Successive British governments consciously retained and used substantial powers of review and intervention to protect Africans from the actions of settlers, to alleviate legislative and administrative discrimination, and, generally speaking, to remove Africans and their problems from the white-dominated political arena. Indeed, it is not usually appreciated that until the mid-1930's the Department of Native Administration was responsible directly to the High Commissioner for South Africa and the Secretary of State for the Colonies. But imperial compromises eroded this exercise of oversight, and Africans, when they read Palley's treatise, will appreciate the extent to which

they must, paradoxically, be both thankful for British protection and critical of the way in which Britain failed to utilize its unquestioned powers as fully and as progressively as it might. In this mixed spirit the British government permitted land apportionment, sanctioned an unduly restrictive franchise, created the federation, gave away much of its reserved power in 1961, and then found itself confronted by the audacity of independence.

Palley's treatment of Rhodesia's legal history is thorough. Although necessarily institutional, her narrative never loses sight of the human element. Despite the fact that I am credited with a bibliographical reference to which I can lay no claim, her book seems free from any but very minor errors. She has, however, often relied for interpretations too uncritically upon the evidence of participants and certain secondary sources. Her book also makes somewhat arid reading. It is, nevertheless, the most important of the numerous volumes on Rhodesia that have been published during the last twenty years.

Harvard University

ROBERT I. ROTBERG

REIZE IN DE BINNEN-LANDEN VAN ZUID-AFRIKA: GEDAAN IN DEN JAARE 1803 DOOR W. B. E. PARAVICINI DI CAPELLI, KAPITEIN AIDE DE CAMP, BY DEN GOUVERNEUR VAN DE CAAP DE GOEDE HOOP. Edited with a summary in English by *W. J. de Kock*. [Van Riebeeck Society Publications, Number 46.] (Kaapstad: Van Riebeeck-Vereniging. 1965. Pp. xxxiii, 290. R3.)

PUBLICATION of the Paravicini manuscript brings to three the number of printed accounts of Governor J. W. Janssens' tour of the Cape during the early months of Batavian rule in 1803. Paravicini, a young artillery captain and the governor's aide-de-camp, compiled the official journal that was published in 1932 by the Linschoten Society. A colonist, Dick Gysbert van Reenen, who accompanied the party, kept a diary that was published in 1937 as Number 18 in the Van Riebeeck Society's series. This third account is Paravicini's personal journal, and it closely resembles the official version. The author, who later served as chief of staff of the artillery in the Netherlands, shared his superiors' concern for the welfare of the colony's non-Europeans, especially the Hottentots, and his journal shows this as well as his feelings for the Boers, whom he felt had been slighted by Barrow. The problems in traveling to the far reaches of the colony are given due attention, but the author also provides, somewhat briefly, his impression of the Xosas, of the negotiations with Gaika, and of the recent destruction on the eastern frontier. One wishes, however, that Paravicini would have made his private notes a fuller supplement to the official journal. The volume also contains his account of a shorter journey to Saldanha and St. Helena Bays in May 1804.

In maintaining the high quality of the society's publications, W. J. de Kock, editor in chief of the *Suid-Afrikaanse Biografiese Woordeboek*, provides a text that is greatly enriched by his careful editing. The supplementary material and the explanatory footnotes, in themselves a good source of biographical data, add much to the publication's value. The English summary, which incorporates the corrections pointed out by the editor in his presentation of the original manu-

script, follows the structure of the text and includes the author's major observations.

Mount Holyoke College

LESLIE CLEMENT DULY

SELECTIONS FROM THE SMUTS PAPERS. Volume I, JUNE 1886–MAY 1902; Volume II, JUNE 1902–MAY 1910; Volume III, JUNE 1910–NOVEMBER 1918; Volume IV, NOVEMBER 1918–AUGUST 1919. Edited by *W. K. Hancock* and *Jean van der Poel*. (New York: Cambridge University Press. 1966. Pp. xiv, 663; v, 638; v, 688; 461. \$12.50; \$12.50; \$12.50; \$10.00.)

THE content of this fine series of volumes is of great historical interest, their arrangement and editing are impeccable, and their format is excellent. This is only half of the series; other volumes will follow. The first four volumes of selections run parallel to, and serve as the main documentary basis for, the brilliant and authoritative biography by Sir Keith Hancock, *Smuts: The Sanguine Years, 1870–1919*. The documents are not merely part of the life of a great man and his country; they also illuminate the history and thinking of his age. Through three wars and several periods of peace, Smuts continuously played an important and constructive part. No one had a more significant role in some of the great transitions of his time: the Union of South Africa; the transformation of Empire into Commonwealth; and the beginnings of an organized world community. The publication of the Smuts Papers on so great a scale is a unique tribute, not accorded to any other statesman of the overseas Commonwealth, and that alone is a measure of their interest. Volume I takes the story to the end of the Boer War, ending with the full text of a brilliant piece of historical writing by Smuts, never before published, his unfinished "Memoirs of the Anglo-Boer War." Volume II covers the peace that ended that war, the period of reconstruction, self-government in the Transvaal, and Smuts's great part in the making of the Union. Volume III has a first draft of a chapter on holism, the outbreak of the Great War, Smuts's campaigns in Southwest and East Africa, his work at the Imperial War Conferences, and his work as a member of the Imperial War Cabinet from 1917 until November 1918. Volume IV, perhaps the most interesting of all, covers his great state paper on the League of Nations, his work on the making of the Covenant, and his long and futile fight to secure a recasting of the Treaty of Versailles.

The four volumes contain papers that are partly private and partly public, 1066 in number, drawn from some 45,000 selected and indexed papers in the Smuts Collection. For the period after the Union, there lies outside that collection a body of as yet inaccessible papers "locked up in the official files." How important the official files can be is shown by an episode in the life of Smuts that has gone unrecorded in all the biographies. This was his participation in the Imperial Defence Conference of 1909, though his name figured in the list of delegates published at the time. This was his first Imperial Conference, and he was getting his first massive contact with the Commonwealth as a whole. Delegates from every member country were working together on the first great joint enterprise undertaken by the Commonwealth; its arrangements for combined

defense were soon to be put smoothly into operation in August 1914. Smuts made an important speech, given verbatim in the printed secret minutes. The whole episode must have left a powerful imprint on his receptive mind, and it supplies a clue to the sudden flowering of his conception of the Commonwealth in 1917.

Bethesda, Maryland

H. DUNCAN HALL

Asia and the East

THE CENSORIAL SYSTEM OF MING CHINA. By *Charles O. Hucker*. [Stanford Studies in the Civilizations of Eastern Asia.] (Stanford, Calif.: Stanford University Press. 1966. Pp. viii, 406. \$10.00.)

THIS is one of those books that makes obsolete a long tradition of amateur comment and apologetic interpretation. It provides a close look at one of the Chinese Empire's characteristic institutions in a period when its powers and functions were fully matured. What we find is neither a channel for the expression of the popular will nor a blind instrument of a despotic state but an organ dedicated to the maintenance of the Confucian state system, with its built-in tension between monarch and bureaucrat. And Professor Hucker makes us see, in the thousands of cases he presents, what manner of men operated the institution: the courageous and the weak, the rash and the cautious, the opportunists and the high-minded idealists, the wise and the cunning.

The book is divided into two principal parts. One deals with the history of the censorate, the Ming censorial establishment and its techniques. The other takes up two periods, one of relative tranquillity (1424-1434), the other of chaos and impending collapse (1620-1627), and examines in detail how the censorate functioned. There are a final chapter of interpretation and twenty-three tables which provide statistical support for his argument. The two illustrative periods are the heart of the book. Here Hucker describes in detail the problems of the time, the character of the reigning monarchs, and the activities of the censors in their two roles of surveillance and remonstrance. In these chapters we are taken into the working of the imperial government, into the great questions of policy, and into the *causes célèbres* that wracked the capital; the careers of many censors are given in detail. Indeed, these chapters offer an extraordinary close-up of Ming political history, particularly valuable for the second of the two periods when the complexities of factionalism on the eve of dynastic collapse are enough to daunt all but the most determined researcher. Hucker relies heavily on the *Shih-lu* [Veritable Records], but draws on many other sources.

The functioning of the censorate in the two periods offers some instructive contrasts: in the first, censors were often rebuked by the emperor for laziness and lack of initiative; in the second, the feeble emperor cried, "Don't harass me," as the censors poured out remonstrances and impeachments, until he turned the government over to the notorious eunuch Wei Chung-hsien; then all but the most intrepid of the censors became silent, while the opportunists among them erupted with praise for Wei and with plans for squeezing more money out of the

people for his extravagant projects. In the first period, censors often came up with proposals for alleviating the people's condition; in the second, they spent themselves on factional warfare or sycophantic effusions. In the first, the censorate produced reasonable recommendations of policy, while in the second, partisanship and a weak emperor favored the hounding down of all those who attempted to solve the Empire's problems. The most dramatic case is that of Hsiung T'ing-pi, Ming commander in Manchuria who fought two wars, one against the Manchus and the other with the censorate. He lost both.

Chapter 1 provides a splendid review of the Chinese censorial system from Han times down to the Ming. Here Hucker sorts out the many strands in this tradition, tracing the growth of separate offices for remonstrance and surveillance, then the various stages at which the two functions coalesced, and the recurrent tendency to assume other functions—judicial, administrative, and military. This is lucid and careful analysis and, together with the main portion of the book, gives a full picture of the “guardians of the law and the customs,” as they performed their privileged but hazardous role in good times and bad. This is reading of high importance for students of comparative institutions and for all who are concerned with the evolution of the Chinese imperial system.

Yale University

ARTHUR F. WRIGHT

THE LIFE AND THOUGHT OF CHANG HSÜEH-CH'ENG (1738-1801).

By *David S. Nivison*. [Stanford Studies in the Civilization of Eastern Asia.] (Stanford, Calif.: Stanford University Press. 1966. Pp. ix, 336. \$8.50.)

CHANG Hsüeh-ch'eng was a scholar who, though successful in the Chinese civil service examinations, never held a government office himself, but eked out a sometimes precarious living as teacher, secretary, or writer of local histories under a succession of official patrons. He was quarrelsome, dogmatic, self-assertive, and politically authoritarian. He was also a highly original historiographer who, through his writings, first brought serious attention to the Chinese local history or gazetteer as a significant historical genre, who advocated the preparation of indexes and other modern devices as aids to historical study, and who above all looked to history for an underlying meaning, in contrast to his contemporaries whose historical approach was narrowly factual and philological. In a series of notable essays he developed a philosophy of history that had to wait until the twentieth century for serious study and appreciation.

To the study of this man Professor Nivison brings a happy combination of Sinological competence and firm grounding in philosophy and history. With exceptional lucidity and insight he has traced the evolution of Chang's thinking through his lifetime, fitted this into the broader social and intellectual context of eighteenth-century China, and shown how Chang, with all his originality, nevertheless remained bound to the intellectual conventions of his age. Little criticism can be made of Nivison's always interesting exposition, save that the general reader might have profited from more precise and detailed explanation of certain frequently recurring topics in the book, such as the eight-legged essay or the nature of the Chinese local histories that so occupied Chang. In view of the

book's length and detail, a concluding chronological table of the main events in Chang's life and his numerous writings might also have been helpful.

On the technical side, the index, while extensive, omits many Chinese terms mentioned in the text. Far worse is the complete absence of any Chinese characters—an inexcusable omission in a scholarly work of this level and one in no way compensated for by the haphazard listing of numerical references to H. A. Giles's Chinese-English dictionary for a few Chinese terms.

This book breaks new ground in the study of Chinese intellectual history between the sixteenth and the nineteenth century. Its excellence points to the great need for further biographical studies of this kind and arouses hope that some of them will come from Nivison himself.

University of Pennsylvania

DERK BODDE

CONFUCIAN CHINA AND ITS MODERN FATE. Volume III, THE PROBLEM OF HISTORICAL SIGNIFICANCE. By *Joseph R. Levenson*. (Berkeley and Los Angeles: University of California Press. 1965. Pp. ix, 180. \$5.00.)

The Problem of Historical Significance brings to a close Professor Levenson's erudite and penetrating trilogy on China's intellectual transition from the essentially Confucian society of the first half of the nineteenth century to the Communist-directed society of the second half of the twentieth. The three volumes overlap in time, and they occasionally deal with the same episodes or persons, but each has its own focus. *The Problem of Intellectual Continuity* (1958) considers "how and why, during so much of Chinese history . . . new ideas [have] had to face tests of compatibility with received tradition, while in more recent times tradition has had to face tests of compatibility with independently persuasive new ideas." *The Problem of Monarchical Decay* (1964) examines "how and why . . . monarchy and bureaucracy [have] been so intimately involved in the Confucian view of culture that abolition of the first, and transformation of the second, have rendered partisans of the third more *traditionalistic* than *traditional*." The present volume deals with differing concepts of historical interpretation in traditional China, in the period of transition, and in the People's Republic.

The interpretation of history accepted by the orthodox Confucianist was that it provided a paradigm that could and should be applied in the present. But this belief weakened during the second half of the nineteenth century as traditional models failed to meet the challenge of aggressive Western civilization. One Confucianist, K'ang Yu-wei, sought to revise the tradition, to read into history the means by which China might accommodate itself to the modern world, but he failed to carry his fellow Confucianists with him. Another, Liao P'ing, gave up the official career expected of a *chin-shih* and devoted his life to preaching his own version of Confucianism, which was neither authentic historically nor appropriate for his own time.

After the 1911 Revolution had destroyed the monarchy, the new historians began to look at the Confucian classics not as bases of historical judgment and action or of ultimate wisdom but as historical documents—the raw materials for "scientific study." The iconoclasts of the 1920's, fighting against the dead hand

of the past, which they believed had too long held China in a position of backwardness and humiliation, felt compelled to condemn much of China's history. But Communist historians since 1949, recognizing that Confucianism is dead and that the greatest threat to their program is the competing ideas and values that had been introduced into China from the non-Marxist West, have been able to take a much more detached view of Chinese history.

In fact, the historians of the People's Republic have turned to Chinese history for support for their own doctrines. During the 1950's they devoted themselves to establishing Chinese periodization along general Marxist lines, even though it took some doing to equate Chinese history with that of the West and at the same time to show that the stages of Chinese advancement were independently Chinese. Although Confucius was rejected by all revolutionaries in the 1920's, the Communists have reinstated him in the 1960's. He and other selected figures illustrate historical stages within the Marxist theory of process, and some of their sayings can be used to support Communist ideas and programs. Moreover they form part of the national tradition and so have patriotic significance.

Levenson has analyzed most helpfully the transition of concepts of historical significance from the Confucian conviction of the absolute applicability of the fixed standards of classical Chinese antiquity to the relativistic Communist historiography under which the past may be either used or rejected as best suits Communist requirements. This book does not make for easy reading, but it amply repays the effort demanded.

Cornell University

KNIGHT BIGGERSTAFF

SCIENTISM IN CHINESE THOUGHT, 1900-1950. By *D. W. Y. Kwok*. [Yale Historical Publications, Miscellany 82.] (New Haven, Conn.: Yale University Press. 1965. Pp. xii, 231. \$6.75.)

THIS small but impressive volume is an excellent introduction to a major aspect of Chinese intellectual history during the first half of the twentieth century: the cult of science that developed into scientism. Professor Kwok defines scientism as "that view which places all reality within a natural order and deems all aspects of this order, be they biological, social, physical, or psychological, to be knowable only by the methods of science." The book presents a clear and systematic analysis of the ideas and arguments of the principal modern Chinese thinkers who advocated or opposed "scientific method" in philosophy, history, and social ideology. It is a case study in the diffusion of ideas from the modern West to a non-Western culture and thus should interest many historians other than China specialists.

An introductory chapter concisely summarizes fifty years of intellectual controversy started by Wu Chih-hui and ended by Mao Tse-tung. Thereafter the expository pattern introduces each thinker with a brief intellectual biography and analyzes his ideas under three categories: conception of science, critique of traditional Chinese civilization, and philosophy of life based upon science. This pattern provides coherence and allows ready comparison among the various ideologists, who enter the stage in chronological order.

Kwok begins his detailed presentation with Wu Chih-hui, a seminal thinker so far as modern China is concerned, who has been little studied in the West. A lifelong revolutionary and supporter of Sun Yat-sen from 1905 onward, he was a philosophical materialist who saw all aspects of life belonging to a natural order and moving according to definite scientific laws. His view was received with enthusiasm by a generation of young Chinese intellectuals. The better-known Ch'en Tu-hsiu, who founded the Chinese Communist party, was, in the author's view, the publicist and vulgarizer of philosophic materialism taken to its dogmatic extremes. Sun Yat-sen scarcely figures as a systematic thinker, although he was touched by the cult of science. Though certainly influential, he is neglected in this book.

The author's admiration goes to Hu Shih and Ting Wen-chiang (V. K. Ting), one a philosophic pragmatist and the other an eminent geologist. He devotes two chapters to them and their coterie who advocated empirical science. He then analyzes the great debate that raged in 1923 between proponents of "science" and "metaphysics" as a basis for a philosophy of life. The empiricists seemed to win the day. Then the subject of debate shifted to the nature and history of Chinese society. "Scientism," in the mold of Marxian historiography, triumphed. Epistemology became the handmaiden of social revolution for a group of historians in the 1930's and 1940's. The debates on Chinese society and history are only cursorily explored by Kwok.

Mao's quasi-philosophical efforts, "On Practice" and "On Contradictions," appealed to youth by then saturated with a scientific outlook; his ideas prevailed because his armies won the revolution, thus "vindicating" theory.

This outline of some major themes does not do justice to Kwok's careful scholarship nor to the clarity of his thought about the major ideas of modern Chinese thinkers. The book is valuable for its excellent bibliography and its biographic information. With this work the writing of modern China's intellectual history has moved a long step forward.

Columbia University

C. MARTIN WILBUR

FORMOSA: A STUDY IN CHINESE HISTORY. By *W. G. Goddard*. ([East Lansing:] Michigan State University Press. 1966. Pp. xvii, 229. \$7.50.)

A GENUINE need in the area of modern Chinese historical literature is a comprehensive history of Formosa based upon fundamental research and presented in accordance with accepted standards of social science methodology. This work, disappointingly, does not fulfill this need. While it is written in an attractive and popular style, often interspersed with poetical and rhetorical renderings, it suffers from dogmatic commentary, an untenable thesis, and numerous factual errors. The bibliography is brief, and only a few lengthy quotations are identified in the text.

Questionable interpretation, such as these representative quotations, requires little explanation to the historian of East Asia: Completely overlooking internal politics concerning the Bakufu-western daimyo imbroglio and the former's preoccupation with trade, Goddard claims that "the policy of the Dutch East India Company

in Formosa was closely linked with the Japanese crusade against Christianity at the time." Again ignoring internal Japanese politics, he claims that when Perry "opened" Japan "the Emperor emerged from his seclusion at Kyoto, and with him came the ambitions [imperialism] which had been closeted for many years." Another questionable causal relationship is his claim that "the Republic of China, inaugurated at Nanking in 1911 [*sic*], had been born in Formosa in 1895." And much attention is given to Sun Yat-sen's brief tenure in Formosa (1900), the author claiming that "Formosa was to be his source of inspiration."

Goddard views Formosa as an entity that has in the past influenced, and will in the future influence, other parts of Asia, particularly mainland China. To support this contention he claims that important mainland developments have been inspired by the model of Formosa and singles out the modernization program of Liu Ming-ch'uan (1880's), the republican effort (1895), and Sun's spiritual experience there (1900). Formosa, in its proper focus, can be viewed best only as a fragment of more encompassing events. While the things he mentions precede similar developments on the mainland, Goddard offers no concrete evidence that they are a primary cause.

University of Wisconsin

LEONARD GORDON

INDIA: A SHORT INTRODUCTORY HISTORY. By *Mark Naidis*. (New York: Macmillan Company. 1966. Pp. xiv, 208. \$5.95.)

DURING the last few years there has been a sudden spate of short histories of India written for the general public. Mark Naidis has made a notable contribution to their number. In two hundred pages of highly compressed but clear and readable text he has produced a useful introduction to a long and complex history for the beginning student. Roughly half the book is devoted to a rapid survey of the ancient and medieval kingdoms, and half to the British period, with a concluding chapter on independent India and Pakistan. Throughout the language is simple and straightforward, with a minimum of technical jargon, and the style is easy and flowing. The utility of the book is much enhanced by an unusual and well-chosen set of illustrations of British social life in India. Unfortunately, however, there are no illustrations of Indian art or architecture, apart from two paintings, and there is only one inadequate map.

Unlike many of the other short introductions that attempt a topical approach, and notice the earlier history only as a backdrop for the present, Naidis has confined himself in the main to political narrative and has tried to deal with the ancient and medieval kingdoms on their own terms. In this he has been largely successful. There are no polemics, a minimum of wild speculation, and very few factual errors. At the same time, however, much of Naidis' treatment of events is superficial and unimaginative. There is much colorful description of social life and customs, especially among the Mogul and British upper classes, but almost no extended or systematic analysis of major historical problems. The significance of the ancient barbarian invasions, for instance, is never assessed, nor is the way in which the newcomers were assimilated into the Indian society, with the result that the Rajputs appear on the stage from nowhere in Akbar's time. In similar

fashion Naidis' discussion of British India is top heavy with political narrative. After devoting two chapters to the conquest of India to 1858, he incorporates into one heterogeneous chapter of seventeen pages, entitled "The Impact of the West," everything from railway development to religious reform. While it is perhaps unfair in so short a book to expect detailed treatment of every subject, still five pages on Warren Hastings and three on the whole cultural revival of the nineteenth century hardly seem a fair balance.

University of California, Berkeley

THOMAS R. METCALF

ANCIENT INDIA: A HISTORY OF ITS CULTURE AND CIVILIZATION.

By D. D. Kosambi. (New York: Pantheon Books. 1965. Pp. xi, 243. \$6.95.)

PROFESSOR Kosambi first ruffled the complacent serenity of the academic historians of India with the publication of his *An Introduction to the Study of Indian History* (1956). That work differed from the common run of histories of ancient India in two respects: it was the first serious and sophisticated attempt to interpret Indian history along generally Marxist lines; Kosambi was the first Indian historian systematically to press into the service of historical writing materials from anthropological observations. These two factors, however, tended to become over-compensated when the exigencies of Marxist theory led to the forcing of facts into the dialectic materialistic mold, and anthropological data were overworked to explain historical phenomena separated from observations by a chasm of centuries, if not of millenniums. But Kosambi's work proved to be a much-needed corrective to prevailing trends in Indian historical writing which, until then, had generally ignored social and economic factors affecting the structure of history. The present work substantially reproduces the contents of the work of a decade ago under a new title.

The first chapter makes general observations on the importance of anthropological data in interpretations of ancient Indian history. This history, for Kosambi, is largely social and economic wherein theories on changes in the means and relations of production crowd the canvas to the extent of elbowing out dynastic and political history. He has nothing but contempt for political history as written by academic historians, and this contempt is his undoing. His book, without the benefit of a chronological or political matrix, breathlessly meanders through centuries. His best chapters are on Magadha and the Maurya Empire; his last chapter is rather a disappointing attempt to chronicle the history of post-Maurya ancient India, a period of nearly twelve hundred years, in thirty pages. Entire periods of intellectual and social history are dismissed in a few pages. The academic historian will fail to be impressed with the "historical" content of the book; the general reader, for whom it was presumably prepared, may find the work frustrating at times. Like its predecessor of ten years ago, Kosambi's present work will be found provocative by those who will read him for the first time. To others, it is an old thesis.

Wake Forest College

B. G. GOKHALE

INDIAN FEUDALISM: C. 300-1200. By *Ram Sharan Sharma*. [Centre of Advanced Study in Ancient Indian History and Culture, University of Calcutta, Lectures and Seminars, Number I-A (Lectures).] (Calcutta: the University. 1965. Pp. xiii, 319. Rs. 15.00.)

THIS volume, the first of its kind on this subject, attempts to isolate those factors relevant to the concept of feudalism and then place them within the framework of Indian history. There are those who would question the use of the word "feudalism" at all in describing the Indian experience. Basham uses the phrase "quasi-feudalism," and it is clear that the Indian situation is rather different in important aspects from that of Norman England, Carolingian France, or the Kamakura period in Japan. The term "feudalism" is an unhappy one when applied to India, but perhaps we have no better word to describe this related phenomenon.

In this well-documented study the author has occasionally weighted evidence in his own favor or suggested conclusions not supported by the evidence. For example, a few grants giving Brahmans the right to enjoy earth deposits in a few villages do not necessitate "the transfer of royal ownership over mines." Insecure tenancy rights, imposition of forced labor, levy of additional taxes, or the absence of legal machinery for redress of grievances may indicate an oppressed peasantry or even a system of slavery, both of which may exist without feudalism. The author himself recognizes the difficulty of stating that feudalism was clearly and consciously a feature of the late Hindu period when he points out that the "texts do not provide any ideological basis for political feudalism, which shows that it had not taken any deep roots in the consciousness of the people before the 11th century A.D."

Whether one accepts Dr. Sharma's interpretation of feudalism or not, it is clear that the author has rendered a distinct service and contribution by focusing on the changes and development in land policies during the late Hindu period. His use of information concerning monetary policies, the changing role of the temples, influence of trade and commerce, aspects of forced labor, taxation systems, and the like gives us an extremely useful study for which we are thankful.

University of Washington

JOHN W. SPELLMAN

RAJA MAN SINGH OF AMBER. By *Rajiva Nain Prasad*. (Calcutta: World Press Private. 1966. Pp. xv, 196. Rs. 25.)

Accounts of Man Singh have been handed down by Abu-l Fazl and other Muslim historians, by Rajput chroniclers, by contemporary Jesuit visitors, and by Colonel Tod of the East India Company in his 1832 history, *Annals and Antiquities of Rajasthan*. Dr. Prasad has made critical comparisons of all these and has carefully assessed original sources in the National and Royal Asiatic Society Libraries in Calcutta, the State Archives of Jaipur, and the Rajasthan Oriental Research Library.

On the whole, he shows skill and sureness of judgment in the use of these

materials. Thus, in rejecting Blochmann's and Jahangir's contention that Raja Man Singh had fifteen hundred wives "and not less than two or three children each," he points out that the palace at Jaipur contained only twenty-four apartments for his queens and that there were "accommodations for fifteen ladies only" in the fort of Rohtas which Man Singh remodeled for their residence while he was governor of Bengal and Bihar. Little, however, seems to be added to the argument by the author's unsupported statement that "it is preposterous to imagine that a man can have so many wives."

Whatever his private life may have been, it is clear from Prasad's evidence that Man Singh, as one of Akbar's ablest generals and confidants, played a greater role in the conquest and consolidation of the Empire than most historians have acknowledged. The author's research on the genealogy of Man Singh and his father goes far to explain why they forsook Rajput loyalties and made common cause with the "hated Turk." The record of parricide and fratricide within the ruling families at Amber reveals an atmosphere of political and military rivalry so bitter that partisans did not hesitate to enlist the support of neighboring Mogul governors. It was decisive intervention by Akbar himself on the side of Raja Bharamal, grandfather of Man Singh, that brought the latter to the imperial service.

Art historians will value the detailed chapter on "Man Singh's Contributions to Architecture," documenting in its descriptions of palaces, mosques, and temples the complex interplay of religious and cultural traditions in sixteenth-century India.

Prescott College

D. MACKENZIE BROWN

SHAH ALAM II AND THE EAST INDIA COMPANY. *By Kalikinkar Datta.*
(Calcutta: World Press Private. 1965. Pp. vii, 148. Rs. 25.)

If anyone were to doubt the quality of the best historical writing produced by Indian scholars today, this elegant monograph would bear witness to its maturity and sophistication. It is also significant that Professor Datta writes in clear, straightforward English, although he has used French, Marathi, and Persian sources.

In 1759 the Emperor Alamgir II was murdered. His heir, Prince Ali Gauhar, upon hearing the news, proclaimed himself Emperor Shah Alam II. So began a Mogul reign of forty-seven years, only shorter than those of Akbar and Aurangzeb. The author begins with Prince Ali Gauhar's attempt to arrest the growth of British influence in Bihar and Bengal. He passes on to the Battle of Buxar (October 23, 1764) and the Emperor's restoration at Delhi. Later chapters deal with the British disavowal of Mogul authority in the period between the Verelst and Cornwallis administrations, affairs in Delhi from 1778 to 1785, the suffering of Shah Alam at the hands of the brigand Ghulam Qadir Khan, and the eclipse of Mogul sovereignty.

Shah Alam was not a great man. The real interest in this study has to do with the revolutionary political changes that took place in India in the second half of the eighteenth century as the English, French, and Marathas sought to

exploit the Emperor's weakness. The Emperor's abortive Bengal and Bihar campaigns won him nothing, but the English were afforded the opportunity of exploiting the fiction of Mogul sovereignty to legalize their power. At Buxar the Emperor himself was among the defeated, which led to the company's superintendence of revenue collection in 1765. The importance of this *diwani* grant can scarcely be exaggerated. Burke considered it nothing less than the penetration of the company into the body politic of India. And in the three decades following, the growing influence of the crown and Parliament over the affairs of the company in India is made apparent. All this and more Datta fully documents.

One hardly need add that this is not a book for the general reader. But every student of Indian history will enjoy it, even though it blazes no new trails.

California State College, Dominguez Hills

MARK NAIDIS

GUNTUR DISTRICT, 1788-1848: A HISTORY OF LOCAL INFLUENCE AND CENTRAL AUTHORITY IN SOUTH INDIA. By *Robert Eric Frykenberg*. (New York: Oxford University Press. 1965. Pp. vii, 294. \$7.20.)

ALTHOUGH not the first to grapple with the complexities of local administration in India, this is a pioneering work, especially in its method. Earlier commentaries, notably those by observant officials who wrote in the early nineteenth century, have been diffuse and fragmentary. They are largely unread and neglected by present-day students of politics and government who need most to profit by them. Mr. Frykenberg, by focusing on one district during a short period, lays bare what happens to "government" when it comes in contact with Indian society. Having grown up in this region speaking Telugu and English, he knows the countryside as well in this century as those of whom he writes knew it a century ago. He consequently makes clear the dangers of uncritically transferring to India Western conceptions of "state," "nation," "federalism," "taxation," and of the interrelations of local with higher authority.

After showing that variants of the conditions he is to describe prevailed in Guntur over many centuries and under all rulers, Frykenberg plunges into his subject. Titles of chapters indicate the nature of the work: on revenue systems—"The Zamindari Settlement 1802-37," "The Amani Management 1800-37"; on organization—"The Headquarters 1837," "Field Operations 1837"; on power struggles and corruption—"Disputes over the Succession of a Sheristadar 1837-39," "Collusion of the Circar Servants 1842-45." He is especially good on the role of Maratha Brahmans in administration in this region and in analyzing the intricate webs spun at both "village" and "secretariat" levels to frustrate higher authority. Perhaps the closest analogies in the West might be found in late twelfth-century feudal society, but there are differences.

Though it is common knowledge that the British ruled India with the aid of thousands of Indians of every caste, creed, and profession, this book provides fascinating evidence that British rule was really not British but Indo-British, constantly reshaped by the interaction of two cultures on the thoughts and deeds of everyone concerned with government from the headman of a village to the governor of Fort St. George. The resemblances between the overwhelmingly

Indian-staffed *Huzur* secretariat at Guntur and the preponderantly European-staffed secretariat at Madras are legion; the differences, few. As Frykenberg points out, the notion that the British district officer was always a philosopher-king with a thorough grasp on the reins of power in his district needs thorough revision. In this period he appears more as a dangling and stumbling puppet. Further studies are now needed of other districts in other regions and during other periods. The author is perhaps not emphatic enough in showing that Guntur, from a historical and sociological point of view, is in many respects unique. His chapters appear as separate distinct essays not welded together into a unified whole, and the work suffers from a profusion of detail and occasional repetition, even of metaphors. For Guntur, it will long hold the field; for India, it should prove the first of a series of similar studies by its author and by others who would profit by his example.

University of Pennsylvania

HOLDEN FURBER

INDIAN POLITICAL ASSOCIATIONS AND REFORM OF LEGISLATURE (1818-1917). By *Bimanbehari Majumdar*. (Calcutta: Firma K. L. Mukhopadhyay. 1965. Pp. viii, 477. Rs. 25.)

Dr. B. Majumdar presents a detailed account of the rise and development of political organizations and groups in British India in the formative century, 1818-1917. His aim is to provide a comprehensive account of all such associations and of their manifold activities. He seeks, meanwhile, to demonstrate that many of the political claims sponsored by the Indian National Congress after 1885 were not original to the Congress. As he shows, political groups, including some that had an ephemeral existence, had publicized a number of the demands made by the Congress in the years before its formation.

In so doing, Majumdar rightly reminds us that emerging nationalism in India was in no sense the product of a single strand or group in Indian public life. He has also demonstrated the richness of resources in India for exhaustive study of political ideas and of the organizations that have received all too little academic attention. While the volume lacks a bibliography, the student of modern India who examines the sources cited in the footnotes will find therein frequent references to such little-used documents.

The author also seeks to make a case for the proposition that constitutional development and reform of the Indian legislatures was the product of pressure applied to the government of India by the several political associations described in his book. Whether this view be valid or not, and that remains debatable, his examination of the role of Indian opinion in pressing for political reform is of considerable value.

In a sense, Majumdar has compiled a massive, well-documented, and compendious account of the many organizations that concerned themselves with questions of reform and constitutional evolution. The volume's value for reference is strengthened by the inclusion of two appendixes, one of which reprints the

regulations that governed the election of members to the imperial and provincial councils under the Government of India Act of 1909.

The volume is, however, largely narrative and descriptive and not analytical. When judgments are inserted, they tend to be jaded, at times even almost naïve. Careful editing could have eliminated needless repetition. In certain instances, Majumdar also accepts "evidence" from his sources in an uncritical fashion. The reader must, therefore, use with care some of the material advanced in this book. While it cannot be employed effectively at the undergraduate level, it can be of genuine help in the seminar and will be useful to those engaged in research. Despite the above limitations, I wish to commend Majumdar for his enterprise and his publisher for making the volume available in a good edition at a reasonable price.

Duke University

ROBERT I. CRANE

MINORITY POLITICS IN THE PUNJAB. By *Baldev Raj Nayar*. (Princeton, N. J.: Princeton University Press. 1966. Pp. ix, 373. \$9.00.)

INDIA provides a major laboratory for testing and evaluating various theories and programs of "nation-building" and political development; the key to many of the developments and trends there is to be found on subnational levels. This volume is one of the best studies of Indian state politics within the framework of the larger implications.

The focus of the study is on the Shiromani Akali Dal, a unique religious-political organization that has dominated the politics of the Punjab, even though it has had little success at the polls in competition with the ruling Congress party. Since 1947 the political conflict between the Akali Dal and the Congress has kept Punjabi politics in "a condition of perpetual crisis." In this conflict the Akali Dal has been "the prime mover," and the Congress "has merely reacted and responded."

The main political demand of the Akali Dal has been for Punjabi Suba—a separate Punjabi-speaking state. Behind the political demand is a more deep-seated objective: "the protection of the Panth," "the protection of Sikh rights and ensuring the Sikhs' continued existence as an independent entity." This demand has deep historical and social roots, as is brought out clearly in this detailed study and in the recently published second volume of Khushwant Singh's *A History of the Sikhs* (1966). It has been pressed by the Akali Dal through a variety of political strategies, all of which Baldev Nayar describes in detail. It has been strongly opposed by the Congress party, by the Hindus of the Punjab, and even by the Harijan Sikhs. Yet in 1966 the Congress party yielded to the demand and announced its decision to create a Punjabi-speaking state in those areas of the Punjab where Punjabi is the predominant language.

Nayar describes at length the caste and other divisions in the Punjab, the basic issues involved in the struggle for Punjabi Suba, the relations of the Akali Dal to the Shiromani Gurdwara Parbandhak Committee, the various political

strategies of the Akali Dal, the main Akali leaders, and Congress leaders in the Punjab. An interesting chapter entitled "Support for the Political System" explains the continuing political support of the Congress party in the Punjab, in spite of Akali agitation and appeals.

The author is thoroughly familiar with the politics and the social divisions of the Punjab, and he is well trained in the techniques of political analysis. He has produced a first-rate book, which may well provide a model for other studies of state politics in India and of nation building in other newly independent states of Asia and Africa.

University of Pennsylvania

NORMAN D. PALMER

THE MAKING OF SOUTH EAST ASIA. By G. Coëdès. Translated by H. M. Wright. (Berkeley and Los Angeles: University of California Press. 1966. Pp. xvi, 268. \$6.00.)

Coëdès' *Les Peuples de la Péninsule Indochinoise* (1962), of which this is a translation, is both a sequel and an addendum to his earlier and longer *Les États Hindouïses d'Indochine et d'Indonésie* (1948). At numerous points the new work either copies verbatim or paraphrases passages from the 1948 book. The author omits references to Malaya and Indonesia, includes a fresh new discussion of Sinicized Vietnam, and extends the story of the principal peoples of continental Southeast Asia into modern times. The prehistoric period is treated in scholarly detail, and scattered paragraphs describe artistic modes and literary achievements of successive periods. The translation is well done save for a few careless errors, some copied from the French text.

The author's most important contribution is his account of Vietnamese history from prehistoric times, a topic on which he is uniquely competent. He also adds substantially to the history of Siam, less so to Cambodia and Laos, and very little to the story of Burma. He seems overly concerned at times with political minutiae. He has little to say of the *montagnards* except to identify them as descendants of prehistoric Negrito and Indonesian inhabitants, whose more progressive kinsmen long since moved southward under pressure of Mongoloid newcomers. He discusses in detail the perennial feuding between the Dai Viet and the Chams and the repeated efforts of the Chinese to recover control of Annam. He points out significantly the sharp ideological divergence between the freedom-loving Thai of northern Siam and the traditionally autocratic Khmer type of government adopted after 1350 by Ayudhya. References to Burma's history are generally superficial and sometimes confused, reflecting no serious research commitment in this area on the part of the author. The post-1800 history of all the peoples runs very thin. He covers Siam from Rama III to Chulalongkorn (1824-1868) in three pages, Mindon's reign to the British conquest of Burma (1852-1885) in twenty-nine lines, and Vietnam's history from 1802 to 1867 in twenty-five lines.

Whatever Coëdès writes is important to students of Southeast Asian history, but his essential contributions thus far relate to the pre-European history of peoples residing east of the Menam Valley.

Ohio University

JOHN F. CADY

A SHORT HISTORY OF MALAYSIA. By *Harry Miller*. (New York: Frederick A. Praeger. 1966. Pp. 274. \$6.00.)

THIS is another history of Malaya, several of which have appeared in recent years. After a brief introductory chapter on the climate, geography, and the Hindu period of Malaya's history, the detailed account begins with the establishment of the kingdom of Malacca in the early fifteenth century. The events of the fifteenth to the late eighteenth century are then covered in 35 pages; the following 190 pages are devoted to the British regime and the transition to independence. The formation of Malaysia, the breaking away of Singapore, and Indonesian confrontation are summarily dealt with in a disappointingly brief eleven pages. The history of the portion of Malaysia in Borneo begins with the establishment of British rule in the nineteenth century. The book deals almost entirely with political events: the entrepôt trade, for instance, is hardly mentioned although it was the principal reason for the foundation of Penang and Singapore, and very little is said about the tin and rubber industries. There are a select bibliography, index, and adequate maps. No analysis is given of the causes of the communal hostility between the Malays and the Chinese, although this is as much the fundamental weakness of the Federation of Malaysia as it was of its predecessor, the Federation of Malaya.

The author, a journalist who spent most of his life in Malaya, frankly admits that he has done no original research and has based his book on the works of the standard authorities on Malaya. It would seem that he has read most, though not all, of them. For the post-1945 period he obtained information from many of the actors in the events. His style of writing is vivid and interesting, and where available he includes extracts from the writings of contemporaries.

This book can be recommended for members of the general public who want a fuller knowledge of the antecedents of the present Federation of Malaysia. They will find a history that in general is both accurate and interesting, even though it does not often go below the surface.

Wolfville, Nova Scotia

LENNOX A. MILLS

NEW GUINEA: THE LAST UNKNOWN. By *Gavin Souter*. (New York: Taplinger Publishing Company. 1966. Pp. 296. \$7.95.)

THE TRAUMA OF DECOLONIZATION: THE DUTCH AND WEST NEW GUINEA. By *Arend Lijphart*. [Yale Studies in Political Science, Number 17.] (New Haven, Conn.: Yale University Press. 1966. Pp. xi, 303. \$7.50.)

UNTIL the 1880's Europeans knew only the fringes of the large island of New Guinea. So difficult of access was its interior that it was not until the rest of the world had been explored that New Guinea was penetrated by a series of explorations that have continued until recent years. In *New Guinea: The Last Unknown*, an Australian journalist presents the history of these explorations and very brief accounts of the administration of their respective territories by the Dutch, Germans, and Australians. A chapter that analyzes the Dutch-Indonesian dispute over West New Guinea and Australia's interest and role in it is of very

great interest and value, as is also the last chapter which deals with Australian problems and policy with respect to Papua and the trust territory of New Guinea. A selected but extensive bibliography, a helpful chronology, and numerous illustrations enhance the value of this interesting and significant contribution to the history of the region.

In *The Trauma of Decolonization* a young Dutch scholar examines the West New Guinea dispute to ascertain whether subjective forces or objective interests played the leading role in Dutch resistance to relinquishing control over this territory. The answer to this question in this particular case would seem to be so obvious as to have made this arduous inquiry unnecessary. A better case for this kind of study would have been the struggle over Indonesian independence. The attitude of the Dutch toward West New Guinea, moreover, can be understood only against Dutch resentment over the forced withdrawal from Indonesia.

Fortunately, the great value of this study lies not in the proof or disproof of any theory about colonialism. It presents a detailed analysis of how a nation in a decade shifted from a passionate determination to retain a colonial remnant even at the cost of war to an almost complete indifference to the issue. Of primary interest to political scientists is the superb analysis of the formation of public opinion on an important issue and the actual making of policy at the top by decision makers. For the historian this study is also valuable. It gives an account of the dispute over West New Guinea which, from the viewpoint of world politics, had many important angles and involved many powers as well as the United Nations.

The study is divided into three parts: "Background," with chapters on colonialism and decolonization, the international setting, and the Dutch and Indonesian arguments; "West New Guinea: Asset or Liability?" with chapters on economic value and strategic and political value; and "New Guinea in Dutch Politics," which is divided into seven chapters and accounts for two-thirds of the book. Dr. Lijphart has digested an enormous amount of material to produce an excellent study.

University of Kentucky

AMRY VANDENBOSCH

ECONOMIC ENQUIRY IN AUSTRALIA. By *Craufurd D. W. Goodwin*.

[Duke University Commonwealth-Studies Center, Publication Number 24.]

(Durham, N. C.: Duke University Press for the Center. 1966. Pp. xv, 659. \$12.50.)

CRAUFURD Goodwin has undertaken the same type of investigation in this book that he did in his *Canadian Economic Thought: The Political Economy of a Developing Nation 1814-1914* (1961), a study of the reception and adaptation of economic ideas and methods of inquiry in a society as it emerges from pioneering colonial to early national status. That he has discovered a richer lode in the antipodes is indicated by the much larger size of the more recent book. Pushing the terminal date from the outbreak of World War I to the crash of 1929 does not by itself account for the threefold increase.

In the larger, first section of the book, entitled "Theory and Policy," Goodwin

examines the economic theories explicit and implicit in the continuing debate on the merits of free trade versus protection, the disposal and taxation of land, the function and regulation of banking and currency, and the development of transport. In addition to these themes, which were also covered in his Canadian study, he has added chapters on economic fluctuations, the function of the state in economic growth, the impact of social Darwinism, the role of labor in the economy, and population studies.

In the second section, headed "The Science of Enquiry," he discusses the development of economic statistics, a field in which civil servants in Tasmania, Victoria, and New South Wales were in the vanguard in the later nineteenth century, and comments on Alfred De Lissa's independent development of the theory of the multiplier in the 1880's and on the advanced monetary theories of the colonial governor, Sir Anthony Musgrave. The slow acceptance of economics as a fully accepted discipline within Australian universities is then traced, with appraisals of the contributions of pioneer lecturers and professors.

The author has relied heavily on separately published material, books, pamphlets (including published lectures), and parliamentary papers, and on periodical articles for his material. Although I must agree with Goodwin's own admission that his failure to undertake an equally thorough search of the newspaper files, parliamentary debates, and the manuscript resources of the Australian archives possibly produced some imbalance, the net he cast covered a wide enough area to permit a reasonable sample to be taken. The result is a commendable overview that should earn a place in bibliographies of more than twenty titles on essential works on Australian history. His footnote references would have provided a more helpful bibliographical aid had first references been more consistently indexed. No bibliography is appended, a false economy in a book of this nature.

It is reassuring to discover that among the economists there are still some who can write history understandable to those not initiated into their mysteries.

University of Waterloo

K. A. MacKIRDY

ALFRED DEAKIN: A BIOGRAPHY. In two volumes. By *J. A. La Nauze*. ([Carlton:] Melbourne University Press; distrib. by Cambridge University Press, New York. 1965. Pp. xiv, 346; vii, 347-695. \$22.50 the set.)

THESE two volumes represent many years of painstaking research. Besides the great collection of Deakin's private papers and writings, Professor La Nauze has exhausted all the public materials. The result is more than biography; it is Australian and British imperial history as well. Occasionally, there is too much detail for a particular incident.

Alfred Deakin (1856-1919) was a cabinet minister in Victoria, a father of federation, three-time Prime Minister of the new Commonwealth, a great orator, and a true statesman. He was also an intellectual and a mystic. La Nauze has the skill and the acute analytic ability to relate the "outer" to the "inner" life of reading, writing, reflection, and prayer. Deakin's private diary and deeply personal comments, including prayers, are revealing as they are wisely and delicately handled by La Nauze. His prayers of the 1890's, for example, show great self-doubt

and self-reproach. The author also brings out Deakin's vast range of interests and associations. He was a journalist, a lawyer, a philosopher, even a spiritualist for a time, and he read and wrote incessantly. He wrote, for instance, a fascinating account of the federal movement that was published posthumously.

La Nauze observes that a key to Deakin's political life was that "policy was more important than party; an admirable sentiment, but one which in practice was likely to lead to courses puzzling to less flexible minds." A cabinet member before he was twenty-four, he worked well in coalition governments, a hallmark of Victorian and early Commonwealth political history. La Nauze probes deeply into Deakin's character, revealing an introspective nature (which drew him to mysticism) and an eclectic religion. His recorded prayers are interpreted as expressing "not a religious system, but a personal faith." La Nauze has the courage to conjecture and speculate on what shattering action or incident might have weighed on Deakin's mind and caused feelings of guilt. He has much to say about Deakin's family and friends, and he illuminates some little-known phases of his public life by reporting such incidents as the meeting of Deakin and Josiah Royce. Judicious selections of Deakin's speeches, together with samples of his ventures into journalism with Australian and English newspapers, enliven the narrative.

Letters to Deakin are historically revealing, for instance those of Isaacs and Higgins when they were appointed to the High Court. Deakin's private diary exposes his mental struggle following his illness in 1907 and his subsequent decline in health.

La Nauze writes with vigor, clarity, and distinction about an important, distinguished, and gracious political figure in Australian history. This, surely, is a definitive work.

University of California, Irvine

SAMUEL CLYDE McCULLOCH

Americas

PRESENCE OF THE PAST: A HISTORY OF THE PRESERVATION MOVEMENT IN THE UNITED STATES BEFORE WILLIAMSBURG.

By *Charles B. Hosmer, Jr.* (New York: G. P. Putnam's Sons. 1965. Pp. 386. \$7.50.)

THIS book fills a need, long felt by scholar and layman, for a comprehensive history of preservation and restoration in the United States. Until Mr. Hosmer published his study, no one was fully aware of the complexities of the subject or able to view it in proper perspective. A segment of the movement in one locality during a short span of years seemed to have little or no relation to another aspect elsewhere in another decade. Only a running glance at the diverse sources used by the author is necessary to appreciate the problems of research that confronted him, and amid the abundance of material he hews to the theme set forth in each chapter. If in some passages the account seems unduly detailed, the reader never loses his sense of direction; nor will he find elsewhere on any portion of the subject such helpful correlation of events with historical background.

As a contribution to cultural history Hosmer's study touches upon many aspects of American social and intellectual life that take on further significance because historical preservation has meant many things to many different Americans. Monuments have long identified historic sites, and markers, historic buildings, but preservation and restoration demand a keener sense of the past to sustain a continuing project in the indefinite future. To what extent have they been an expression of community spirit, patriotic pride, or private enterprise manifesting itself independently in one locality and seeking a common denominator with the public interest in another? In instance after instance Hosmer demonstrates the amateur, splintered, and disorganized nature of the preservation movement, whether it be the saving of Mount Vernon, a noble example that occupies a whole chapter, or the acquisition of the old Seventh-Day Baptist meeting house by the Newport Rhode Island Historical Society, recounted in a paragraph. The chapters with a regional focus point up some striking contrasts between public and private support. The movement, he declares, "was an American response to an American need," and no characteristic is more noteworthy than its diversity.

Hosmer has wisely ended his account on the eve of the Williamsburg restoration. Since 1926 a new era has emerged with large-scale organization, expanding financial investment, the advent of the museum field and American historical archaeology as recognized professions, and the phenomenal development of tourism since World War II, aiding and abetting three-dimensional history. Some of these factors are anticipated in the chapters on antiquaries and architects, techniques of restoration, and the economics of preservation. And now his essay, written in 1963, on "Private Philanthropy and Preservation," in *Historic Preservation Today* (1966), may serve as both summary and supplement to his excellent book.

Institute of Early American History and Culture

LESTER J. CAPPON

HIDDEN HIERARCHIES: THE PROFESSIONS AND GOVERNMENT.

By *Corinne Lathrop Gilb*. (New York: Harper and Row. 1966. Pp. viii, 307. \$5.95.)

MEMBERS of the American Historical Association have special professional reasons for considering Dr. Gilb's ideas about the power of the professions today. First they can judge her supposition that American society is far advanced in a nine-century cycle toward becoming, again, what Western society was in the Middle Ages, a status society. The "work revolution" and the "consumer revolution" of our own times, she says, making comparisons with the Industrial Revolution during the nineteenth century, have vastly increased the professions. They have subdivided the old professions into more and more complex specializations; they have called new professions into being; they have multiplied the memberships, both in absolute numbers and in proportion to the increase of the population. Most important of all in the present book, they have enlarged the problems and mechanisms of the professions' own self-government and their role in public government. Professional organizations are the guilds of the modern age; as naturally as the medieval guilds, they enter the educational process and ultimately the processes of

sovereignty. Second, besides such general matters, historians can gain an impression from Gilb's collective portrayal of how their own degree of progress and power through organization compares with the rest of the field.

The author's tastes, training, and experience are very broad: in political theory, in American civilization, in law, and possibly in sociology. By contrast she has worked little in any of the specializations common to academic history. At any rate her argument about the Middle Ages seems to me loose and unpersuasive (not unimportant or uninteresting), as do other long-range positions taken for perspective—a passage, for example, that likens today's professional organizations more to churches of congregationalist polity than to episcopal ones. On the other hand, her statistical and political data concerning the big three professional organizations—the American Bar Association, the American Medical Association, and the National Education Association—and concerning the major and minor organizations in their total impact in her own state of California are detailed and convincing. Though I would prefer less jargon and less adjectival accounts of tendencies, and more nouns and verbs giving concrete illustration, she lands her main points. In about thirty years, we learn, a profession with a reform to effect in American society can become more than a pressure group, indeed a real and not very “hidden” participator in our modern federalist “continuum of government.”

Historians, be alert! You have power to gain if you seek it. You have little to risk but the old individualism, the old equality in association, and the old casualness of operation within the guild.

Johns Hopkins University

CHARLES A. BARKER

MEDICINE IN AMERICA: HISTORICAL ESSAYS. By *Richard Harrison Shryock*. (Baltimore: Johns Hopkins Press. 1966. Pp. xviii, 346. \$7.50.)

DOUBTLESS some will feel that a collection of essays in book form pleases many, but really satisfies few. Shryock, however, has written enough monographs for the few. In this fine book for the many he has assembled sixteen essays written over the course of thirty-five years on the general subject of American medical history. Topics covered include a general history of American medicine, medical practice in the Old South, and a splendid article on Civil War medicine. The author says much in small compass. We next learn the significance of Sylvester Graham and why his only monument is a cracker. Two essays deal with the various public health movements and the Tuberculosis Association. Two articles on the profession itself follow. Four essays consider medical thought and research, with some emphasis on Philadelphia. No better discussion of the controversial Benjamin Rush has been written. Equally good is the piece on the remarkable Cotton Mather. Three essays on historiography end the book. One of these, a plea for the study of the history of science as a means of encouraging American pure science, written in 1944, seems dated considering the flowering of pure science here since the war. The final essay is a valuable summary of the history of medicine. The author's lively style and wit prevent even the least interesting sections from being dull. In the majority of the chapters hardly a paragraph fails to provoke thought

and a desire for research. The fourteenth chapter, "Medical Sources and the Social Historian," could be studied with profit by all general historians who ignore the impact of medicine and health on society. This neglect of the central point of all life and all societies is hard to understand. The author tries to remedy this attitude in many of the essays.

This well-documented, well-indexed book ought to be read by all physicians if they can find the time, by all medical students who should take the time, and by all general historians that they may be enlightened.

Fredericksburg, Virginia

GORDON W. JONES

APOSTLES OF THE SELF-MADE MAN. By *John G. Cawelti*. (Chicago: University of Chicago Press. 1965. Pp. xiv, 279. \$6.95.)

As a discipline history does advance, and more systematically than critics sometimes assume. Professor Cawelti's study of changing concepts of success in America builds on earlier work, incorporating the best of the old and pressing on to the investigation of the new. The author is too busy with his own constructive synthesis to quarrel with his predecessors or trumpet their failings to the world. His own forte is interdisciplinary synthesis, the kind often praised but little practiced in the American studies field. Though he relies heavily on novels and other formal literature and on the techniques of literary scholars, he also draws on the data and insights of historians and social scientists. The result is a many-sided and richly textured treatment of the ideas that have clustered around the figure of the self-made man.

Because it illuminates many competing definitions of success, and is more concerned with their variety than with the singularity and persistence of a few dominant themes (rags to riches, for example), this study has more range than its predecessors. Its free structure permits easy exploration of the relationship of ideas of success to other social traditions, among them the traditions of self-culture, of gentility, of mobility, and of social responsibility.

With allowances for overlapping in chronology and content, the author identifies three historic patterns of the ideology of success. The first, deriving from middle-class Puritanism, assumed a hierarchical social order in which success meant the attainment of a modest competence in this world and salvation in the next. That view persisted into the nineteenth century, where it mixed with a more secular, individual, and dynamic tradition that equated success with great fortunes, industrial progress, and unlimited mobility. In the twentieth century a more benign value system emerged. Owing much to Jefferson and Emerson, it equated success with personal fulfillment and social responsibility, rather than with wealth and upward mobility. Though he gives the nineteenth-century devils their due, the author likes the third tradition best.

Because novels serve as the principal sources, and leading writers as the main vehicles of presentation, the reader sometimes feels that he has seen this stable of horses before, under other riders and on other tracks. Hugh Henry Brackenridge, James Fenimore Cooper, Henry James, and Theodore Dreiser are tired old nags

even under the spur of self-made men. But thoroughbreds get across the finish line in a hurry under Cawelti's crackling whip and skillful ride.

University of Wisconsin

IRVIN G. WYLLIE

PIONEER AMERICA. By *John R. Alden*. [The History of Human Society.] (New York: Alfred A. Knopf. 1966. Pp. xxix, 309, x. \$6.95.)

"The History of Human Society" is the ambitious title of a series of volumes written by established scholars for the general public. As explained in a provocative introduction by the editor, J. H. Plumb, the series has both a theme and a purpose. The theme is "the growth of man's control over his environment," and the purpose is "to restore a little confidence in man's capacity not only to endure the frequent catastrophes of human existence but also in his intellectual abilities." Of the volumes that have appeared so far, John Alden's *Pioneer America* best illustrates both theme and purpose.

This book begins with a generous chapter on the American Indian and concludes with the Civil War settlement in 1865. The early colonists brought with them the marks of their Anglo-Saxon backgrounds, Alden points out, and by 1700 English domination of the North American continent was nearly a foregone conclusion, although the struggle with France would continue for another half century. Almost from the start Americans were a wealthy people compared to their European cousins. Inspired by a conviction that they had a special mission to lead the way to a better life, they conquered a continent rich in natural resources and in time made the American dream an attainable goal for millions of people.

The triumphs achieved by Americans along the way are better known and understood by the general public than are the setbacks. The major catastrophe of American history is of course the great schism that began to open up in the early nineteenth century between the supporters and opponents of Negro slavery. Alden judges abolitionists and southern extremists with equal harshness and cites the unwillingness of northerners to offer a plan of compensated manumission as a major factor contributing to the crisis. He believes that, had adequate compensation been offered, southern leaders like Robert E. Lee could have convinced fellow slaveholders to free their Negroes. Alden seems to reject the view of many historians that the real barrier to manumission was not economic but social, that most southerners defended slavery as the only way to "keep the Negro in his place."

Alden's concluding chapter is a fine overview of the present condition of America as seen in the perspective of its pioneer period. Here is a book that gives us the mature judgments of one of our most respected historians, a book written graciously, and one that provides its readers a broader and deeper meaning for the first two centuries of American society.

Williams College

BENJAMIN W. LABAREE

A RELIGIOUS HISTORY OF AMERICA. By *Edwin Scott Gaustad*. (New York: Harper and Row. 1966. Pp. xxiii, 421. \$8.95.)

I HAVE been told that when Charles de Gaulle was asked what one of his pro-

nouncements would mean to the ordinary Frenchman, he replied, "I do not know. I am not an ordinary Frenchman." My attitude toward this book is analogous. I suppose that in this area I am not an ordinary reader.

But as a teacher of mixed classes made up of graduate and undergraduate, "ed" and "co-ed" students from every area of the university who are to be introduced to "Religion in American History" I welcome it. It seems to me that Mr. Gaustad, already acclaimed for his *Atlas of Religion in America* (1962), has produced the most imaginative and original survey that has appeared. Written with the touch of a scholar acquainted with the field, the work also reflects experience in communicating the ideas of history in the classroom. A survey, it is one of "secular" American history's first fruits, exemplifying what Professor Henry F. May so ably delineated as its "Recovery of American Religious History" (*AHR*, LXX [Oct. 1964], 79-92).

The work skillfully combines a topical and chronological presentation, approximately two-thirds being given to the period since the Revolution, when "liberty" was won, proclaimed, and secured, and one-half to the era since the Civil War ("This Nation under God: At Worship," and "At Work"). The author ingeniously interpolates 224 apt illustrative quotations from sources in the text itself, and this, plus the 301 illustrations, gives a feeling of immediacy throughout. The "Suggestions for Further Reading," trimmed to "volumes that are generally readable and widely available," put into the hands of even the novice teacher in the area a useful guide to further development. The book gives due and balanced consideration to the modes of belief, worship, and practice of the three traditional faiths of our country, Protestant, Catholic, and Jewish, and makes clear how each fits into the religious history of America.

Although one may differ and even quarrel with some of Gaustad's emphases and interpretations, the work is refreshingly free of irritating errors.

University of Iowa

SIDNEY E. MEAD

THE NORTHERN COLONIAL FRONTIER, 1607-1763. By Douglas Edward Leach. [Histories of the American Frontier.] (New York: Holt, Rinehart and Winston. 1966. Pp. xviii, 266. \$7.95.)

THIS book is one of a planned eighteen-volume series of "Histories of the American Frontier," edited by Professor Ray Billington, each volume telling "the complete story of one phase of westward expansion." This considerable enterprise will provide the first up-to-date analysis of that extremely vital and pervasive interpretation of American history inaugurated many years ago by Frederick Jackson Turner. If this volume is typical, we may expect a re-examination, though not necessarily an overturning, of all of Turner's generalizations, which in turn may set in motion an entirely new group of monographs comparable to those based on Turner's teachings, written and unwritten.

Professor Leach's primary purpose is to describe the interaction between the earliest settlers who came to the North Atlantic coastal region bringing with them a pattern of culture developed by the demands of their earlier environment and a wilderness sparsely inhabited by aborigines whose pattern of culture had

been developed by their environment. By thus defining the area called the "frontier," Leach is able to sort out and organize an integrated set of facts and generalizations which, though not in themselves novel, provide a new account of the early coastal settlements that is stimulating to read, every page compelling the reader to rethink the usual chronological versions of our early history in terms of the complex interaction of forces, which is his theme. There will undoubtedly be disagreement with a number of the author's conclusions (about Indian relations, for example), but one of the values of the book is that every page suggests a subject for more detailed treatment.

The brevity of the volume emphasizes the problems of selection and organization. The balance between narrative and analysis is nearly even: seven chapters may be said to "tell the story," and five give background and descriptive detail. "The Life of the Pioneer," "Land Speculation," and "Christianity" are among those which have been singled out for separate treatment. In these last, particularly, the specific detail is often unfamiliar, being "mined" from a wide range of sources, in itself a testimony to the author's vast research. Occasionally, phrases like "a large part of the northern frontier," "tribe after tribe," "hundreds of settlers," "other such Christian villages . . . formed in other parts of the colony" seem to be unnecessarily vague.

One must admire the skill with which the author has worked his way through the early history of the Northeast, selecting enough detail to explain but not to clutter his main theme. One relatively minor motive for the exploration of the wilderness that seems to have been omitted is the search for mineral wealth. Again "scalping parties" but not "snow-shoe men" are mentioned in connection with defense. It is significant that there are few references to Britain or to British policies.

The bibliographies are excellent; one would find it hard to suggest more than a half-dozen omitted printed references. The illustrations are well chosen, and there are seven maps. The device of showing the "direction of expansion" by the use of arrows and dates seems to me unsatisfactory; the names of the places being settled might well have been included. The map of "speculative tracts" is incomplete. For example, nearly all of New Hampshire was a speculative tract based on the Masonian claim.

Boston University

ROBERT E. MOODY

THE OVERSEER: PLANTATION MANAGEMENT IN THE OLD SOUTH. By *William Kauffman Scarborough*. (Baton Rouge: Louisiana State University Press. 1966. Pp. xv, 256. \$7.50.)

THOUGH the overseer was central to the operation of the plantation economy of the ante bellum South, this book is the first systematic study of his role. J. S. Bassett's book of forty years ago rested on a single set of plantation records; Mr. Scarborough has examined more than seventy. He has also made excellent use of manuscript census returns and agricultural periodicals. Rather than undertake the perfectionist task of covering every southern state, he has studied the overseer in seven states that grew tobacco, grain, rice, sugar, and cotton. The result is a

thorough, clearly written, and probably definitive study of the overseer in the ante bellum South. Except for a chapter dealing with the overseer during the Civil War, the book is topically, rather than chronologically, organized.

Some of his conclusions will not surprise readers familiar with the general literature on plantation management. By differentiating among overseers in the four staple areas, he substantiates Lewis Gray's observation that the more competent and fortunate overseers were concentrated in the rice and sugar areas, where skill and knowledge were essential. He also documents the overseers' discouragement, which derived from the planters' distrust, interference in plantation management, and social snobbery. Suggestive of one source of discontent among overseers, which Scarborough brings to light, is his finding that about 40 per cent of the overseers were not married, though their average age was thirty-three. On the whole, the author is on the side of his subject and, along with earlier authorities, thinks overseers were grossly underpaid, considering their great responsibilities and skill. He does not seem equally confident of his conclusion that "it is likely that the majority of . . . overseers treated the Negroes in their charge fairly well."

Both his statistical and manuscript sources reveal a somewhat higher economic position for the overseers than previous works have assigned to them. Many overseers, he concludes from his manuscript sources, managed to move up to farm ownership, and even those who did not accumulated some property; a few even died wealthy. He makes no comparison, however, with the economic status of southern farmers in general; nor do his sources provide any basis for judging the social sources of overseers. His explanation for the generally low reputation of overseers, aside from the conflict in interests between overseer and planter, is that there was always a class of "drifters" or incompetents among the overseers who, while not a majority, were sufficiently numerous to discredit the whole profession.

Scarborough's conclusion that "slave discipline was clearly the decisive factor in the success or failure of an overseer" needs more emphasis than it receives in the book, for it helps to account not only for the conflicts between planter and overseer but also for the difficulties that faced the South in improving its agriculture. A good agriculturist might well be a poor disciplinarian, but he could not survive as an overseer in the system; a mediocre or poor farmer who could handle slaves, however, had a place, even as he compelled the frustrated planter to keep seeking that ideal manager who was good at both parts of the crucial job.

Vassar College

CARL N. DEGLER

GRAVEN IMAGES: NEW ENGLAND STONECARVING AND ITS SYMBOLS, 1650-1815. By *Allan I. Ludwig*. (Middletown, Conn.: Wesleyan University Press. 1966. Pp. xxxi, 482. \$22.50.)

RELIGIOUS art springs from a "burning need for imagery," Allan Ludwig argues, and such an art may be found in the tombstone carvings of the New England Puritans. By interpreting the symbolic meaning of that art and illustrating it with excellent photographs, Ludwig reveals a new area of cultural interest. Obviously the Puritans possessed deeply felt needs which the rationalistic formulas of

orthodoxy could not satisfy and which sought expression unconsciously, despite their traditional iconoclasm, in images that transcended logic.

Ludwig contends that the carvings prove the persistence of piety in rural areas long after it is presumed to have declined in the coastal towns. Here he seems to equate religious enthusiasm with piety; yet, as he indicates earlier in his discussion, Puritan piety rejected enthusiasm. One wonders, then, how the carvings could be outlets for suppressed enthusiasm and, at the same time, expressions of continuing Puritan piety. May it not have been, in fact, the decline in piety in the eighteenth century that resulted in a weakening of church restraints that made possible "graven images"? Religious identification of the people for whom the stones were carved might help clarify this point, as would some indication of their feelings about the designs. The remarkable thing about a New England graveyard is the similarity of the stones, suggesting not individual tastes or religious devotion but simply what the carver had in stock. The standardization of the stones is more impressive than the occasional individualization we find and suggests fashion rather than belief.

By trying to relate New England gravestone art to Puritan religious symbolism and belief, Ludwig has confused his argument; nor has he helped it with a prose that is sometimes obscure. As a revelation of an important source of vernacular art, however, his book is important. What is of particular interest is his discussion of the similarity between these carvings and the folk art of other early cultures; the sources of the New England style and what they suggest about the transit of civilization from England to the New World; the inner development of the styles as they underwent modification by stone carvers working in relative isolation without the benefits of a craft tradition; and the "American propensity" for a plain style, with emphasis on line, geometric effect, and abstraction.

University of Wisconsin-Milwaukee

LILLIAN B. MILLER

THE EARLIEST DIARY OF JOHN ADAMS: JUNE 1753-APRIL 1754; SEPTEMBER 1758-JANUARY 1759. THE ADAMS PAPERS. Series I, DIARIES. DIARY AND AUTOBIOGRAPHY OF JOHN ADAMS: SUPPLEMENT. Edited by *L. H. Butterfield et al.* (Cambridge, Mass.: Belknap Press of Harvard University Press. 1966. Pp. xx, 120. \$3.95.)

It was thought that the tremors of an earthquake in 1755 had prompted John Adams to commence his diary, but we now learn that he began while under the influence of John Winthrop during his second year at Harvard. To what must have been their initial dismay, the editors of the *Adams Papers* discovered a fragment of Adams' diary among the Royall Tyler Collection in the Vermont Historical Society only a few months after the publication of what was believed to be a definitive edition. More power to them, for they have given us a fascinating volume in its own right. To be sure, this is, as Butterfield describes it, "a disorderly miscellany or catchall" of student notes, letter drafts, and legal case notes, but we know too little of Adams' college years and legal studies not to be grateful for this "fragment."

Tyler's courtship of young Abigail Adams won greater sympathy from her

mother than from an absent and fretful father who, with considerable reluctance, gave permission for Tyler to make use of his legal notes and library. It is surmised that this section of the diary disappeared in this way. When John was assured that Tyler's legal practice and material prospects boded well, the purchase of a handsome Braintree property being taken into account, he consented to a courtship that eventually came to nothing. Tantalizing glimpses of his own romantic interest in Hannah Quincy give vitality to this volume, and we learn much about him between eighteen and twenty-three as he moved from Harvard to his first legal case: something of his respect for scientific inquiry, more of his often morbid introspection, and much of his lust for fame and the mortification that failure in his first courtroom appearance brought with it. Not the least of his virtues, however, was the determination to persist in whatever he found worth while. He would decide what his talents and training fitted him for, what the shortest route to that goal was, and what skills must be acquired in order to travel it successfully. What a profane pilgrim and what a dull affair his life might have been had he not been blessed with vision—the law seen as “the whole Circle of Science and Literature, the History, Wisdom, and Virtue of all ages”—and with such overpowering curiosity about the world around him. We see in him the yearning and the faith to see life whole. From Winthrop's lectures on the laws of motion to his own attempts to deduce the laws of politics and of human behavior in the seclusion of his first law office is a short span.

Butterfield's introduction to the volume provides a fine biographical sketch, and the same high standards of editing that have marked all the volumes in this series have been maintained.

Institute of Early American History and Culture

STEPHEN G. KURTZ

THE CHANGING POLITICAL THOUGHT OF JOHN ADAMS. By *John R. Howe, Jr.* (Princeton, N. J.: Princeton University Press. 1966. Pp. xv, 259. \$6.50.)

ONE of the problems of John Adams' political thought is the seeming irrelevance of so much of it, the latter half in particular, to American society of his day. There was a terrible abstractness about it. The conventional categories he employed took little account of the uniqueness of the American situation, and his favorite theoretical terms—“orders,” “balance,” “aristocracy,” “passion for distinction,” and so forth—somehow failed to discover true significance in the stuff of real life. John Howe argues in this perceptive book that the answer to the riddles of Adams' thought lies not so much in the prepossessions of intellectual tradition or in the directives of political experience as in his poignant sense of changing personal relationship with the American people.

Howe endeavors to understand Adams' thought as a product of this relationship. Hence, his approach is experiential: Adams' ideas are closely tied into his practical experience as a political leader. Indeed, the political and constitutional ideas themselves receive only passing attention as the author, instead, uncovers the moral assumptions that underlay them and charts their changing course in the context of the statesman's career. The emphasis is on change. Howe subscribes

without qualification to the view associated with Adams' Jeffersonian critics that his thought underwent a "sea change" in the 1780's and was thereafter fixed in its direction. In the earlier period he believed in the peculiar virtue of the American people and identified the patriotic movement that gave him fame and applause with love of liberty and passion for the public good. During the 1780's, however, he came to believe that the American people had changed, grown quarrelsome, selfish, spineless, and lawless, until there was little to distinguish them from other societies. If the reassessment owed something to Adams' observations while in Europe, it owed much more, Howe thinks, to a host of frustrations and embarrassments in his official position that led to feelings of rejection. He returned from Europe in 1788 with a revised set of attitudes toward American society, and these would be further strengthened by events at home and abroad. Indeed, Europe and America became blurred in his mind. Seeing in America the same corruptions he had seen in Europe, he concluded that the same political principles were applicable here as there. Only then did he divide society into competing "orders," each identified with a different principle and function of government. Whereas before Adams had relied on the reason and virtue of the people to keep society in order, he now turned his attention to instruments of authority and control to check the overreaching ambitions of both "democracy" and "aristocracy." And since none of these concerns proved very realistic, it is no wonder that after the final climactic defeat in 1800 Adams felt like "an archaic survivor from a past age."

This is a sensitive portrayal of a deeply tormented mind. In the final analysis, it is not very complimentary to Adams, who, says Howe, "tended to develop a simple equation between his personal interest and the well-being of American society." The implications of this judgment are not developed here. The book is a sketch, highly thematic and suggestive, and the scholar who may in the future undertake a full-bodied study of Adams' political thought will surely be indebted to it.

University of Virginia

MERRILL D. PETERSON

FRONTIER MISSION: A HISTORY OF RELIGION WEST OF THE
SOUTHERN APPALACHIANS TO 1861. By *Walter Brownlow Posey*.
(Lexington: University of Kentucky Press. 1966. Pp. viii, 436. \$9.00.)

PROFESSOR Posey, the author of books on Methodists, Presbyterians, and Baptists, has now written a more general book, including accounts of the Disciples of Christ and the Cumberland Presbyterians, as well as Roman Catholics and Episcopalians. Most of his attention is directed to the Southwest from the Revolution to the Civil War, but he touches occasionally on developments in South Carolina and Georgia, comments on the northern scene when necessary to explain the great separations, and includes a few pages on Catholic efforts in the area in the seventeenth and eighteenth centuries. The categories of his analysis are pretty much those made familiar by William Warren Sweet, to whose memory Posey dedicates this book: camp meetings and circuit riders; the emergence of new denominations and the first efforts of old ones like the Episcopalians; the estab-

lishment of various kinds of church government; the abortive efforts to Christianize the Indians, and the carefully avoided concern for Negro souls; attempts to build schools and seminaries, and to reduce smoking, drinking, and swearing; the growing polarization of attitudes toward slavery; the breakup of several denominations.

What most distinguishes Posey's book, besides its unrivaled command of detail, is his obvious unwillingness to assume that what men who called themselves churchmen did in their churches or in the name of their churches constituted "religion" in any important sense. Whether in his infrequent references to theological change, in his discussion of missions, in his accounts of the failure of most educational ventures and moral reforms, or in his delineation of the developing religious defense of slavery, Posey presents an extremely strong case for the epiphenomenalism of the "religion" he is studying. He does so somewhat reluctantly, and he concludes with a magnificent affirmation from Isaiah. But if the argument for the importance of religion in American culture is to rest on more than rhetoric, it must, in my judgment, be made in the context, not of church history, however clear eyed, but in the perspective of a broader history in which the fullness of men's economic, social, and sexual passions are juxtaposed with "religious" behavior.

Columbia University

ROBERT D. CROSS

GEORGE RAPP'S HARMONY SOCIETY, 1785-1847. By *Karl J. R. Arndt*. (Philadelphia: University of Pennsylvania Press. 1965. Pp. 682. \$12.00.)

WITH their friends, the Shakers, the Harmonites (Rappists) were by far the most successful and renowned of the scores of religious communitarian groups that have flourished in the United States. In recent years serious historians have begun to recover the history of the better-known groups. The Shakers, for example, have been particularly well served in the lifelong collecting and writing of the late Edward Deming Andrews. But because the majority of the sources for the history of the Harmony Society are in German, many of them in Swabian dialect, the Harmonites have been neglected, and students of American history have had to depend on the ancient and quite incomplete doctoral dissertation (1904) of John A. Bole.

For the past twenty years interested historians have awaited the monumental study by Karl J. R. Arndt. As a professor of German and the son of a Missouri Synod Lutheran missionary, Arndt is steeped in the language, culture, and religious tradition of the Pietism that gave birth to the Harmony Society. His densely detailed narrative, the first of two projected volumes, covers the history of the movement from the European origins to the death of "Father" George Rapp in 1847. The chief contribution of the work lies in the enlightening effect of the many new details Arndt has amassed around familiar topics: German origins, land purchases, theology, agricultural and manufacturing enterprises, the physical aspect of the three Harmonite towns, internal crises, and so on, all copiously illustrated by maps, paintings, and photographs. The most original section is the long

and thoroughly researched narrative of the remarkable career of Rapp's would-be rival, Count Leon, the "Lion of Judah."

Arndt's strong pro-German and pro-Pietist bias, solidly grounded in research, provides an excellent corrective to several traditional distortions, especially the common dismissal of the Harmonites as slavish, ignorant, avaricious, inarticulate German peasants completely outside the mainstream of American history, but his bias also leads him to exaggerate the influence of the Harmonites and to make irritatingly belligerent statements about the superiority of German culture and the Pietist religion.

I can find only two serious faults with this important and valuable work. First, perhaps a third of the text consists of source material quoted *in extenso*, including even such things as a long list of all the "noble pioneers" buried in the New Harmony cemetery. Second, for so ambitious and definitive a work of over six hundred closely printed pages, the documentation is almost ludicrously inadequate: nine pages of not very helpful footnotes, not one of which hints at the whereabouts of one of Arndt's most important sources—the supposedly lost letter books of Rapp. This lack of documentation is a most unfortunate and fundamental shortcoming, which may or may not be remedied in the promised second volume.

University of Massachusetts, Amherst

MARIO S. DE PILLIS

THE PRISONERS OF ALGIERS: AN ACCOUNT OF THE FORGOTTEN
AMERICAN-ALGERIAN WAR 1785-1797. By H. G. Barnby. (New York:
Oxford University Press. 1966. Pp. xii, 343. \$7.50.)

ALMOST as soon as the United States became an independent nation its government had to face the problem of piracy in the Mediterranean. Light, swift, armed ships from North Africa's Barbary shore preyed on American merchantmen trading in the Mediterranean. The Muslim captors enslaved the American seamen, put them to hard labor in rock quarries and elsewhere, and kept some of them as domestic slaves. Since American merchants and shipowners found the Mediterranean trade lucrative, they were unwilling to give it up or to curtail it. To continue the trade unmolested they had either to pay tribute to the Barbary States or obtain naval protection from their government. They turned to their government for help.

In the period 1785-1795 the strongest and largest of the Barbary powers, Algiers, captured more than a dozen American ships and enslaved over a hundred Americans. One of these slaves, an aggressive and shrewd young Irish-American sailor who had fought in the American Revolution, James L. Cathcart, adjusted to his captivity so well that he rose to the position of Chief Christian Secretary to the Dey of Algiers. Algiers waged war against the United States, but there were no organized hostilities or major battles. The pirates of Algiers merely captured American ships and demanded ransom and gifts for American prisoners.

This book, by an English writer, is a popular account of the earliest effort of the United States to come to terms with the pirate states of North Africa through diplomacy. The theme is ineptness. Various American diplomatic representatives

carried on long fumbling negotiations to obtain release of the enslaved seamen. Hassan Bashaw, the Dey of Algiers, always demanded more than the Americans were willing to pay; he also demanded a high price for a peace treaty. Finally, in September 1795, the Dey made a peace treaty for a price that seemed steep to the Americans but cheap to him. Since the prisoners were not to be released until the ransom was paid, they remained in captivity almost another year. The American government was slow in delivering payment.

Although this book is gracefully written and filled with interesting and colorful details on life and customs in North Africa, such as on the use of Turkish Janizaries and on how the Muslims treated Christian slaves, the scholar will find in it little in the way of information or interpretation that is new. Both the research and the analysis lack depth. The ordinary reader will find in the book the virtues of skillful narration and description.

University of California, Santa Barbara

ALEXANDER DeCONDE

1787: THE GRAND CONVENTION. By *Clinton Rossiter*. [The New American History Series.] (New York: Macmillan Company. 1966. Pp. 443. \$7.95.)

VENERATION of the American Constitution is as old as the inauguration of the United States government, but it reached its apogee in the late nineteenth and early twentieth centuries. For the past half century or so, popular reverence for this "Magic parchment" and "Transforming word" (as a prominent lawyer once described it) has continued, but historians, working along the lines sketched out by Charles A. Beard, have raised a storm over the Constitution, probing the motives of those who wrote it as well as the hidden intent of its most famous clauses. Professor Rossiter's book is a scholarly reflection of those Fourth-of-July orators who perennially have glorified the achievement of 1787. Those "heroes" who wrote the Constitution, Rossiter believes, "made a gamble with the destiny of the American people so hazardous and yet so calculated, so contingent and yet so prudent, that they command the highest homage granted to makers of history: an endless retelling of the manner of their ascent to glory."

The author begins with a survey—economic, political, social, and intellectual—of the United States in 1787 and finds, as have so many before him, that it "was a good country in which to live, work, and aspire." As a backdrop for the great drama enacted in Philadelphia, he next describes the ills that beset the United States in the 1780's, the remedies that were proposed, and the fears of those who opposed change. Before opening the curtain on the "Grand Convention," he provides biographical sketches of its participants, the stars as well as the supporting cast. The four (out of sixteen) chapters that describe the well-known debates and compromises of the convention are, to the professional historian at any rate, the least interesting part of this book. Its last section relates the familiar story of the struggle for ratification of the Constitution and the less familiar story of the "last years of the Framers." It also includes two chapters that might better have been omitted: an appraisal of the Constitution and an account of the history of its first years. The former repeats, for the most part, what the reader already has been

told; the latter appears out of place in a book whose theme is "1787—the year that made a nation."

But taken as a whole, this is a popular and authoritative, spritely and accurate, entertaining and instructive book. This is surely enough to make it the best general history of the Constitutional Convention since the publication of Carl Van Doren's *The Great Rehearsal* in 1948.

Lafayette College

JACOB E. COOKE

THE ORDEAL OF THE CONSTITUTION: THE ANTIFEDERALISTS AND THE RATIFICATION STRUGGLE OF 1787–1788. By *Robert Allen Rutland*. (Norman: University of Oklahoma Press. 1966. Pp. xiii, 329. \$6.95.)

With a prefatory acknowledgment of the pioneering work of Orin Libby and Charles Beard, and a reference to Beard's supporters and opponents, Robert Rutland washes his hands of the historiographical controversy regarding the origins of the Constitution and launches into a straightforward chronicle of its ratification. His work, he declares, is "neither a defense nor an attack on Beard"; thereafter, except for a note regarding the appearance of Jackson Main's *The Antifederalists* when this book was in its final stages, Rutland ignores the controversialists.

Beginning with a brief review of the backgrounds of antifederalism before and during the Constitutional Convention, he turns, before the end of his first chapter, to a chronological account of the struggle over ratification from the close of this convention in September 1787 through the hasty action taken that fall in the three states nearest the seat of the convention to the ratification by Virginia and New York, the tenth and eleventh states, in June 1788. The two final chapters deal with the failure of a movement for a second convention and, very briefly, with the ratification by the remaining states, North Carolina and Rhode Island, and the Antifederalist acceptance of the finality of these events, an acceptance that made possible the domestic tranquillity of the first Washington administration.

The story is a familiar one, but it is freshly told because Rutland writes from original sources, primarily from private correspondence and newspapers. He finds the common ground of opposition to the Constitution to lie in fear of a shift in the locus of political power, and he credits the failure of this opposition to a lack of newspaper support, of organization, of unified leadership, and of a concerted plan of action or a definite counterproposal. The most effective argument of the Antifederalist party was their demand for a bill of rights, but it was countered by the master stratagem of the Federalists, first employed in Massachusetts, the adoption of recommendatory amendments to accompany the act of ratification.

Rutland's work is narrative rather than analytic and, in that respect, is different from the studies by Main and by McDonald. Little is said here about who owns what; no close analysis is attempted of the personnel of the ratifying conventions; and there is no analysis at all of the electorates that chose the convention delegates. But within the pattern he sets for his work Rutland's is the single most comprehensive account of the ratification process.

University of Delaware

JOHN A. MUNROE

SEARCH AND SEIZURE AND THE SUPREME COURT: A STUDY IN CONSTITUTIONAL INTERPRETATION. By *Jacob W. Landynski*. [The Johns Hopkins University Studies in Historical and Political Science, Series LXXXIV (1966), Number 1.] (Baltimore: Johns Hopkins Press. 1966. Pp. 286. \$8.50.)

ONE of man's most cherished rights is the right to privacy. This right is spelled out in the Fourth Amendment of the US Constitution, which forbids "unreasonable searches and seizures." The Supreme Court regards this right as being so fundamental to a system of ordered liberty that it has read it into the due process clause of the Fourteenth Amendment as a federally enforceable restriction upon the states. While the Fourth Amendment was long dormant, it finally found its place in American constitutional litigation with the advent of national prohibition, which raised sharp issues regarding the line between licit and illicit searches and seizures. It has been at the center of a storm of controversy ever since, for it is never easy to find a proper balance between the right of the individual to freedom and personal dignity and the interest of society in enforcing the criminal law.

Professor Landynski reviews the history of the Fourth Amendment, both as a limitation on federal power and as a restriction upon the states, with great skill and superb scholarship. While his own position is clearly on the side of the rights of the individual, he recognizes and does full justice to the countervailing considerations. He reviews the history of the Fourth Amendment and the Court's broad construction of the concept of privacy implicit in it. He gives full attention to the federal exclusionary rule and to its ultimate application to the states, as well as to searches without warrant, eavesdropping, and administrative invasion of privacy through inspection. He recognizes that many "barriers of privacy have crumbled on many fronts" and that the task for the Supreme Court is to try to preserve those which are left.

The remaining barriers are analyzed with an acute perception of the basic problems involved. This is a refreshingly literate and well-researched analysis of a great body of American constitutional law doctrine. It is absolutely free of any sort of quantification that has become so much the fashion in these days when so many social scientists are trying to look like mathematicians. There is not a table or arithmetic equation in the whole book, but there are much good history, intelligent analysis of social problems, and good writing. In fact, this is by far the best book we have on the constitutional law of the Fourth Amendment.

University of Wisconsin

DAVID FELLMAN

THE WASHINGTON COMMUNITY, 1800-1828. By *James Sterling Young*. (New York: Columbia University Press. 1966. Pp. xvi, 307. \$7.95.)

THE weakness of the federal government under the Jeffersonians constitutes the central theme of this book. The title, in fact, might well read "The Washington Non-Community, 1800-1828." For James Young proposes that before the Jacksonian era the prevailing philosophy within the federal establishment rested upon two beliefs, both destructive to effective government: that centralized power in a

republic was in itself evil and that senators and congressmen assembled in Washington to protect their constituents, not to rule the nation. From these premises the text moves on to an analysis of the mechanisms employed on Capitol Hill that defeated attempts at governing the country and the consequences of congressional inability or refusal to seek a consensus on national policies. The role of the Presidents and executive department heads in struggling against or acquiescing in this nonrule next comes under scrutiny.

The over-all discussion demolishes the interpretations of two generations of American scholars who have viewed the Jeffersonian period as one in which party discipline ran strong, at least until John Quincy Adams' presidency. By examining congressional roll calls and caucuses, the correspondence of participants, and the comments of such shrewd foreign observers as Captain Basil Hall and of that perceptive Washingtonian, Margaret Bayard Smith, Young shows that party control on most issues was virtually nonexistent. Jefferson, adroit politician that he was, generally succeeded in winning support for his policies before he encountered fierce popular opposition to his embargo, but after 1808 neither he nor any of his immediate successors was able to grasp the helm firmly enough to guide the ship of state. On the contrary, divisiveness on Capitol Hill prevented the chief executive from so much as presenting a legislative program in orderly fashion.

At several points, for example, in stressing the geographical isolation of Capitol Hill and the deliberately contrived reinforcement of social nonintercourse effected by the congressional boardinghouse groups, the author, in my opinion, overstates his case. The swamp at the foot of the Hill, particularly after the Washington canal was opened in 1815, was not the impassable morass Young depicts. Nor does the term "segregation" fit the social regime of congressional boardinghouse dwellers; the author himself modifies that exaggeration by describing the White House dinners that congressmen attended at Jefferson's invitation. Fortunately such stylistic aberrations and an irritating tendency to repeat every argument three times over do not invalidate Young's findings and conclusions. The book offers an important, long-overdue corrective to earlier misconceptions about the viability of American governmental processes in the United States' early years.

Washington, D. C.

CONSTANCE McLAUGHLIN GREEN

JEW AND IRISH: HISTORIC GROUP RELATIONS AND IMMIGRATION. By *Rudolf Glanz*. [Published with the help of the Alexander Kohut Memorial Foundation.] (New York: n.p. 1966. Pp. 159.)

This volume, which explores historic factors determining the relationships of the Jews and the Irish in the United States, is Glanz's fifth study of historic group relations involving Jews. The author has previously written about Jews in relation to Yankees, Mormons, Germans, Chinese, and to the "Bayer" and "Pollack" in America.

The fact that the nineteenth-century mass emigration from Ireland preceded the Jewish mass exodus from Russia by nearly a generation was, as Glanz asserts, an important determinant of relations between the first native American Irish

generation and the generation of Jewish immigrants largely from Eastern Europe. Thus, although the majority of Jews and Irish in the United States today are native-born citizens, their mutual relations "may have been historically pre-determined by earlier accommodation to the American environment."

In twelve brief chapters the author emphasizes primarily Jewish rather than Irish situations. He touches upon the image of the Jew in European folklore and its effect upon the Irish, Jewish reaction to the Irish "catastrophe-emigration," the meeting of Jews and Irish in the United States and their social, economic, and political relations, Jewish response to St. Patrick's Day, "Jew-Irish" stories in American folklore and humor, the Jew in the Irish-American press, and the "Irish Self-Comparison with the Jew and the American Comparison of Jew and Irish." Although the incidents and attitudes depicted by Glanz are intrinsically interesting, his treatment of them is disjointed, and some chapters are little more than outlines. Frequently the wording is awkward, as if literally translated from the German. There are numerous misprints, twenty-one pages of "footnotes" buried at the end of the volume, and no index.

Glanz's interpretations and conclusions contribute modestly to an appreciation of Jewish-Irish relations, but the book is valuable chiefly for its illuminating anecdotes and quotations from contemporary periodicals and newspapers.

Adelphi University

ROBERT ERNST

EXPLORATION AND EMPIRE: THE EXPLORER AND THE SCIENTIST
IN THE WINNING OF THE AMERICAN WEST. By *William H. Goetzmann*. (New York: Alfred A. Knopf. 1966. Pp. xxii, 656, xviii. \$10.00.)

WHEREAS the eighteenth century witnessed French and Anglo-American exploration and occupation of the territory east of the Mississippi, the nineteenth century was the era of the great American reconnaissance of the Far West. According to Goetzmann, there were three major periods of western exploration: the first, beginning with Lewis and Clark and ending about 1845, was characterized by imperial rivalry in which "the mountain men and the fur traders were self-conscious pawns in an international competition for the West"; the second was a period of settlement and investment in which the explorer "was largely dedicated to lending a helping hand in the matter of Manifest Destiny"; the third, from 1860 to 1900, was a time for scientific inventories and surveys and beginnings of conservation planning. Here, then, is our first comprehensive survey of American western exploration written by an able scholar who has already given us a penetrating volume on western army exploration. In this new volume the reader will find many familiar figures as the author traces the paths of explorers from the time of "the clash of empires" to the heyday of John Wesley Powell. Beginning with Lewis and Clark, the reader follows the trails of Manuel Lisa, Zebulon M. Pike, James Ohio Pattie, Jedediah Smith, Captain Benjamin Bonneville, John Russell Bartlett, Clarence King, Ferdinand V. Hayden, J. W. Powell, and many others. Copious footnotes, a comprehensive bibliography, numerous illustrations and maps, and the readable text demonstrate that the author has canvassed the

great document repositories and libraries on western history. He is, moreover, well informed on the important scholarship bearing on his subject. One of the interesting interpretations Goetzmann sets forth is that the fur trader-mountain man was a kind of money-hungry capitalist, often an obedient fur company employee and pawn of imperial rivals. Hardly a romantic hero, or a member of that "reckless breed," the mountain man represented the climate of expectant capitalism of the Jacksonian era. If Goetzmann sees no romance in these hairy, grubby frontiersmen of the mountains, he finds the genteel, literate, explorer-reformer to be the real hero of the nineteenth century. Hayden was "a towering but typical figure," and Powell was "the greatest explorer-hero since the days of Frémont." Although the author has given special attention to the scientist-explorer, the reader finds occasional omissions, as, for instance, the Swiss-American scientist-explorer of the Southwest, Adolph Bandelier. An untouched body of fascinating correspondence on the Southwest between Bandelier and Francis Parkman is in the Parkman Papers, Massachusetts Historical Society. And what about the omission of Parkman himself, a pioneer in comparative ethnology, who went west to study Indians in 1846? The reader might also question occasional generalizations about the nature of western culture and Turner's hypothesis. But these are minor reservations. Goetzmann has a great canvas, and he has used his opportunity to produce a spirited, challenging book destined to become the standard work on American nineteenth-century western exploration.

University of California, Santa Barbara

WILBUR R. JACOBS

THE JOURNALS OF ZEBULON MONTGOMERY PIKE: WITH LETTERS AND RELATED DOCUMENTS. In two volumes. Edited and annotated by *Donald Jackson*. (Norman: University of Oklahoma Press. 1966. Pp. xxviii, 464, 60 plates; xiii, 449. \$20.00 the set.)

ZEBULON Montgomery Pike was the most daring and adventuresome of American explorers. He was also one of the most successful. His two expeditions cost less than 10 per cent of what was spent by Congress on the Lewis and Clark Expedition. With the appearance of these handsome volumes, Pike has received, at last, the full and fair treatment to which he is entitled.

Much hitherto unpublished material has been included. The editor has used a manuscript copy of Pike's Mississippi Expedition instead of the published version. He has reproduced the rough sketch maps made by Pike on both his explorations. From these we may conclude that, had the maps not been retained in Mexico for a century, Pike would have been recognized long ago as one of the greatest contributors to the geographical knowledge of our country. Dr. Jackson has published translations of all available Spanish correspondence bearing upon Pike's southwestern excursion. None of these documents afford any solid support for those historians who have viewed Pike as a tool or accomplice in the Wilkinson-Burr schemes. Pike emerges as a patriotic officer who kept his eyes open for the benefit of his country while under suspicion and duress at the hands of the Spaniards.

Dr. John H. Robinson, Pike's unofficial companion, is considered to have been

a free-lance adventurer, rather than Wilkinson's emissary to the Spanish authorities. Though his case is less clear than that of Pike, the documents tend to support such an interpretation.

The editor exhibits a familiarity with geographical detail that enables him to correct a few slips of previous editors. He demonstrates that the Pawnee Republican village visited by Pike was in Nebraska rather than in Kansas. There are occasional errors, as when he has Pike descending the Wet Mountain Valley instead of ascending it or when he states that Jackson and Carter were not members of Pike's first expedition, although both are definitely so listed. A view of Pikes Peak from some other direction than the north, which was the only direction from which the explorer never saw it, would have been more appropriate than the one selected for illustration.

Generally speaking, this complete edition of Pike material is a splendid piece of work. It should end effectively the practice, long indulged in by ill-informed writers, of making carping comments upon Pike's career and achievements. It is a distinct pleasure to recommend this work as a highly important contribution to American history.

Colorado College

HARVEY L. CARTER

JOHN C. CALHOUN. By *Richard N. Current*. [The Great American Thinkers Series.] (New York: Washington Square Press. 1966. Pp. ix, 182. \$3.95.)

THIS volume in "The Great American Thinkers Series" is divided into four sections: a short biography of Calhoun; a summary of his political theory; an analysis of the significance and influence of his thinking; and a review of the literature on Calhoun. Though the first two sections form a succinct and admirable introduction to the subject, the second half of the book is more original and more stimulating. No graduate student should miss Richard Current's pungent bibliographical essay, in which he details his reasons for differing with numerous other scholars.

Current is not an admirer of Calhoun; he never has been. At about the time that Charles M. Wiltse and Margaret Coit were publishing their excellent, highly sympathetic biographies of the South Carolina statesman and Arthur M. Schlesinger, Jr., and August O. Spain were portraying him as a champion of minority rights, Current wrote an important dissenting article, bluntly called "John C. Calhoun, Philosopher of Reaction." In a 1955 study of Webster he again argued that Calhoun was not even a good conservative. Now, more fully and more persuasively than ever, he presents Calhoun as a reactionary interested only in justifying and protecting slavery.

The recent vogue of Calhoun, Current maintains, is based upon a misreading of his works; the South Carolinian's scholarly admirers "have attributed to him the very political principles and practices which he detested." He was not a profound thinker, for his writings reveal "gaps, inconsistencies, contradictions, and downright errors." He was not a deep student of the Constitution, for he "found in the fundamental law just what he wanted to find." He was not a defender of minority

rights, for he was concerned for the interests of only one minority: the southern slaveholders. Today only "the die-hard defenders of segregation are thoroughly justified in thinking of themselves as successors and inheritors of Calhoun."

Closely argued and carefully documented, this little book is deliberately polemical. It does not further illuminate the middle period of American history; nor is it intended to. It does not give the reader an understanding of Calhoun as a man, and it does not explain how a man of his high intelligence came to adopt such faulty reasoning. What it does do most admirably is to sweep away old myths and sentimental fantasies, and it points up the need for fresh thinking about Calhoun, which must begin by accepting him on his own terms: southerner, slaveholder, and secessionist.

Johns Hopkins University

DAVID DONALD

GEORGE C. YOUNT AND HIS CHRONICLES OF THE WEST: COMPRISING EXTRACTS FROM HIS "MEMOIRS" AND FROM THE ORANGE CLARK "NARRATIVE." Edited by *Charles L. Camp*. (Denver, Colo.: Fred A. Rosenstock, Old West Publishing Company. 1966. Pp. xviii, 280. \$20.00.)

GEORGE C. Yount was a fur trapper who was fortunate enough to die peacefully in bed and doubly fortunate in that he left a sizable estate and heirs to carry on after him. His experiences included service in the War of 1812, farming, a trip to Santa Fe in 1826, and then about five years of adventurous trapping in the great Southwest. He knew the Patties, Wolfskill, Young, Pegleg Smith, T. O. Larkin, and many other men of importance in southwestern discovery and California settlement. He did well at sea otter hunting and commanded sufficient respect from the Mexican authorities to be given a large grant of land in the Napa Valley. There he raised livestock, developed fruit orchards, harvested grain, and put up honey. He died in 1865.

This book consists in large part of Yount's "Memoirs" coupled with a long, loosely constructed narrative of his life by the Reverend Orange Clark, who recorded Yount's reminiscences in the years 1854-1855. The language and grammar are in the stilted, awkward forms in which they appear in the original documents. In order to achieve a semblance of organization, the editor used his introduction to present a thumbnail sketch of Yount's life; then he devoted Part I to an outline of "Trapping and Trading in the Southwest, 1815-1830," dealt with Yount's life in the next seven parts, and devoted the last two sections to Hugh Glass and Smith.

For anyone interested in the fur trade or early California this book is a prize indeed. It carries with it a flavor of the times, even to the sermonizing of the Reverend Clark; it is full of yarns; it is carefully annotated; and, with its photographs and large map of Yount's perambulations, it is a fine collector's item. Working historians, however, will lament the economy measure of placing the footnotes at the back, since, from the price and physical quality of the book, money was no problem.

Florida State University

RICHARD A. BARTLETT

CALIFORNIA'S FIRST ARCHBISHOP: THE LIFE OF JOSEPH SADOCA ALEMANY, O.P., 1814-1888. By *John Bernard McGloin, S.J.* [Makers of American Catholicism, Volume II.] ([New York:] Herder and Herder. 1966. Pp. 412. \$9.50.)

In preparing this biography of Archbishop Alemany, Father McGloin faced a very difficult task of trying to tell the story of a Catalan bishop whose Spanish origins made him a suitable bishop for early California but whose diocese was going through the tremendous changes of the California gold rush. At times, in the narrative, the reader is not certain which is more important: the growth of the Church under these unusual conditions or the quiet, stubborn prelate whose task was to meet the changing circumstances. In so far as this is a biography, the details of Alemany's early life and his career as a missionary in Ohio and Tennessee are too few.

Undoubtedly, Alemany did not want to be a bishop and certainly had no desire to be the bishop of California, yet his stubbornness in defending the rights of his diocese against the Jesuits and Dominicans is the best testimonial that he took his task seriously. He was evidently a man of great simplicity and a courageous and tireless worker. Perhaps, if the author had used the contents of the chapter on his character in the earlier chapters, those chapters would not seem so barren. Similar observations could be made of his treatment of San Francisco and the other areas of Alemany's apostolate. One does not sense any connection between the prelate and the stormy times of California after the gold rush. McGloin tells quite well the story of the archbishop's participation in the Vatican Council and his search for a coadjutor. But there must have been more personalia available about this small Spaniard who traveled continuously and had to organize a diocesan clergy made up almost entirely of men from other regions. Details from the reports to Roman and mission authorities on the conditions of the archdiocese would have given this volume a more earthy touch that one expects in a biography.

University of Notre Dame

THOMAS T. McAVOY

DUTY, HONOR, COUNTRY: A HISTORY OF WEST POINT. By *Stephen E. Ambrose.* (Baltimore: Johns Hopkins Press. 1966. Pp. xv, 357. \$8.00.)

No undergraduate institution in the United States has had as much written about it and yet is so little understood by civilian educators as West Point. It is, in a real sense, a national institution, established and regulated by the federal government. It is a school steeped in tradition, ingrown, resistant to change, and governed by a small group of permanent professors with congressional tenure. By the standards usually applied to any college or university, it would be found lacking, and David Boroff's caustic characterization of it as "a second-class college for first-class students" is superficially correct.

But West Point cannot be judged solely by these standards. It takes its motto, the title of Ambrose's work, seriously, and has as its chief aim the development of character, leadership, and devotion to duty. Because he judges on this basis,

Ambrose finds little to criticize in the West Point of today and much to admire in its past. His history traces the checkered course of the academy through its early impoverished years and the administration of Sylvanus Thayer (1817-1833), who made it one of the great institutions of the early nineteenth century and model for many of the schools founded in that period.

Between the Civil War and World War I, when the movement for higher education in the United States flourished, West Point stagnated. All efforts to change the school were frustrated by the faculty and the alumni who felt that to tinker with a system that had produced such outstanding leaders as the Civil War generation of commanders would be disastrous. Only after General MacArthur assumed the superintendency in 1919 did West Point begin to change, and all the reforms since then, says Ambrose, stem from MacArthur's brief three-year regime—a judgment I find difficult to accept on the basis of the evidence.

Ambrose's sympathetic treatment of present-day West Point tends to obscure some of the real problems of the academy, such as reform of faculty and curriculum. He apparently overlooked a recent study by William E. Simon, *Liberal Education in the Service Academies*. But his volume has the merit of relating the history of West Point to developments in American higher education. In this respect it is superior to some of the older histories of the academy, although it by no means supersedes Sidney Forman's history, published in 1950. Ambrose draws liberally from reminiscences and manuscript collections to enliven his account with tales of cadet life, hazing, scandals, Negro students, and football, to which he devotes an entire chapter. The volume is marred by some minor errors in names, titles, and dates; two unpublished manuscripts are listed as printed sources, and several works bearing on the history of the academy are not noted. These are not serious matters and will not lessen the enjoyment of the general reader and the thousands of devoted alumni, including General Eisenhower, whose enthusiastic foreword should do much to make the book a success.

Dartmouth College

LOUIS MORTON

THE EMANCIPATION OF THE AMERICAN WOMAN. By *Andrew Sinclair*. [Harper Colophon Books.] (New York: Harper and Row. 1966. Pp. xxix, 401. \$2.45.)

CURRENT discussions of the problems of today's American woman have fortunately been accompanied and possibly modified by studies of the feminist movement in this country and of nineteenth- and twentieth-century ideas of woman's sphere. Recent works that approach the topic as both social and intellectual history include Flexner, *A Century of Struggle*; Riegel, *American Feminists*; and Kraditor, *The Ideas of the Woman Suffrage Movement*. This book, published also in hard cover as *The Better Half*, adds another readable account of women's activities in this country especially between 1820 and 1920 and provides a number of theories to explain the changes that occurred. Perhaps the definitive word on this subject has not yet been written. Some of the points made suggest fruitful areas for additional research. Mr. Sinclair sees woman's emancipation as tied in part to the technological changes of the past century, which brought economic freedom to

women. He is especially insistent on the relation of urbanism with a resultant leisure class of women to the growth of American feminism and stresses, too, the close kinship of antislavery and temperance organizations to the "woman movement." Some adaptations of the Turner thesis appear in his treatment of the alternating prominence of East and West in the struggle, especially in the disagreements of their respective leaders. The author indicates that in the present century excessive attachment to Freudian doctrine has, through stress on sex, produced a devotion to home and maternity rivaling in its way the religiously based views of the mid-1800's. In his final chapter, "The New Victorians," Sinclair, who is sympathetic to the problems of women, turns to exhortation and encourages American women to see themselves not "chiefly as reproductive females" which leaves them "the weaker sex and the better half" but "as human beings" working "for the better whole."

The work draws on a wide range of primary sources and is copiously supplied with footnotes, though students might find a more formal bibliographical arrangement helpful.

Mount Holyoke College

MARY S. BENSON

GEORGE SMITH'S MONEY: A SCOTTISH INVESTOR IN AMERICA. By Alice E. Smith. (Madison: State Historical Society of Wisconsin. 1966. Pp. vii, 197. \$4.50.)

In the summer of 1833 George Smith of Aberdeenshire, Scotland, then twenty-five years of age and possessed of a full measure of native caniness, took passage for New York. Sixty-six years later, a few months before the close of the nineteenth century, he died at the exclusive London Reform Club where he had lived in retirement for nearly forty years. His estate was set at about \$52,000,000. It was remarked in the House of Commons in March 1900 that the death duties from this fortune were more than sufficient to meet the cost of a dreadnought.

In this book Alice E. Smith chronicles the rise of George Smith to enormous wealth. Smith made his fortune in America, in the Midwest, particularly in Chicago. The young Scot had the good sense to be in this future metropolis when it was still a frontier village. He engaged initially in land speculation, later in insurance and banking, and emerged in the 1860's as a large-scale investor in railroads. Smith was the acknowledged leader of a group of Aberdonians, bound together by ties of kinship and common origin, who cooperated closely in various business ventures in America.

From the standpoint of economic history, the most interesting thing about Smith is not that he made an enormous fortune but that he was the originator of what came to be known as "George Smith's money," certificates of deposit issued by his Wisconsin Marine and Fire Insurance Company. In the depths of the depression of the early 1840's when banks were proscribed in Illinois and Wisconsin and yet credit was desperately needed, "George Smith's money" played a vital role. Emanating from an insurance company and essentially illegal, Smith's certificates passed at smaller discount than the notes of most specie-paying banks. For years the Scot successfully fought off attempts by competitors to drive his

money from the arteries of commerce. Apparently beaten in 1853 by an Illinois law forcing him to recall his notes, Smith acquired control of two Georgia banks and had issued \$2,750,000 of bank notes by the end of 1854. Even these notes from a distant state passed in the Midwest at small discount because they had the name and fortune of Smith behind them. In this fashion did Smith, guided by the "Invisible Hand" of another and more famous fellow countryman, provide a gushing well of credit to nourish the enterprise of the American Midwest. To reverse a phrase, by doing well Smith also wound up by doing good.

The author has done a creditable job in setting forth the impact of Smith upon young America's economic development. If he remains a shadowy figure, it is because his whole being was devoted to the making of money with little thought to posterity and historians. If history is indeed essentially melodrama, Smith and his ilk will be and should be remembered only in the prosaic statistics of economic development.

George Washington University

ROBERT P. SHARKEY

FIRST WHITE WOMEN OVER THE ROCKIES: DIARIES, LETTERS, AND BIOGRAPHICAL SKETCHES OF THE SIX WOMEN OF THE OREGON MISSION WHO MADE THE OVERLAND JOURNEY IN 1836 AND 1838. Volume III, DIARY OF SARAH WHITE SMITH (MRS. ASA B. SMITH); LETTERS OF ASA B. SMITH AND OTHER DOCUMENTS RELATING TO THE 1838 RE-ENFORCEMENT TO THE OREGON MISSION. With introductions and editorial notes by *Clifford Merrill Drury*. [Northwest Historical Series, Volume VIII.] (Glendale, Calif.: Arthur H. Clark Company. 1966. Pp. 332. \$11.00.)

DISCOVERY of the diary of Sarah White Smith, of letters of Asa Bowen Smith, and of other papers of the 1838 expedition serves admirably to round out the documentary material on the first Oregon mission of the American Board of Commissioners of Foreign Missions, carefully collected and edited by Professor Drury. The bibliographical history of the documents is well explained, and the contents, precisely annotated.

Mrs. Smith's journal reveals clearly the religious motivation and intense piety characteristic of many members of her generation. Her accounts and those of her male companions describe the hardships and peculiar problems of the trip. The relatively uneventful journey from Westport, Kansas, to Waiilatpu, the Whitman station in Oregon, was made on horseback, the four women riding sidesaddle, from April 23 to August 30, 1838. One reads much of illness, accidents, stray livestock, the fur traders' "Rendezvous," and the hazards of mountain crossing. Mrs. Smith appended a comprehensive and informative list of suitable garments for each sex for the benefit of later travelers. Her desire for dark undergarments suggests problems of propriety as well as laundry. One senses also the extreme overcrowding of the first winter at Waiilatpu and the later extreme isolation in the separate stations. Much information is presented on dissensions within the mission itself. Asa Smith appears difficult and contentious, but the continental crossing and the early months in the field might well have discouraged even a budding

saint. The editor finds in the Smiths' accounts continued support for the theory that Marcus Whitman's famous journey of 1842-1843 was rather to save the mission than to "save Oregon" from the British. The Smiths moved to the Sandwich Islands in 1842, but Mrs. Smith's health had been sacrificed.

Drury tries from contemporary accounts to determine the wisdom of the American Board in sending women overland rather than around the Horn. The preponderance of the evidence condemns this. Nevertheless, as the editor indicates, women had made the trip, and the precedent proved important in the peopling of Oregon.

Mount Holyoke College

MARY S. BENSON

JAMES LUSK ALCORN: PERSISTENT WHIG. By *Lillian A. Pereyra*. [Southern Biography Series.] ([Baton Rouge:] Louisiana State University Press. 1966. Pp. xv, 237. \$7.50.)

LILLIAN Pereyra's *James Lusk Alcorn* is an excellent and thoughtful study of Mississippi's most influential scalawag. Moving to Mississippi in the 1840's, Alcorn quickly established himself as a vigorous lawyer, a wealthy delta planter, and an earnest Whig politician. Originally opposed to secession, he "went with his state" and became a brigadier general of Mississippi troops, but he was never more than a reluctant Confederate. He attacked Jefferson Davis' "stupid policy; his egotism, arrogance, and superciliousness," and he openly traded with the Yankee enemy. At the end of the war, after an unsuccessful attempt to revive the Whig party, he joined the Republicans, who elected him governor and later senator.

Since Alcorn left only sparse personal records, tracing his long career is no easy assignment, and the author has admirably used the surviving Alcorn manuscripts, including some documents still owned by the family. Because the sources are so thin, she has wisely not attempted any deep probing of Alcorn's personality or motives. Necessarily her account of his prewar career is thin, but her chapters on the Reconstruction years are richly informative. From her analysis it is easy to see why Mississippi whites were destined to play so small a part in the reconstruction of their state. Most, who still thought of the Negro as a slave, were shocked by Alcorn's announcement: "I propose to vote with him, to discuss political affairs with him; to sit, if need be, in political counsel with him . . .," and they fled into the racist Democratic party. At the same time the freedmen, together with their radical carpetbagger allies, objected to Alcorn's insistence that "Political equality does not imply by any means social equality . . .," and they were offended by his demand for absolutely segregated schools and transportation facilities. Though Alcorn's governorship was moderately successful, he was unable to build the coalition of Negro and white voters that he needed, and, when the carpetbagger Adelbert Ames defeated him in the gubernatorial race of 1873, all hopes were ended "for a conservative, homegrown Republican organization in Mississippi."

Because Alcorn shifted political allegiance so often, he was vulnerable to charges of inconsistency and opportunism. Pereyra argues that he was, however, always true to certain fundamental Whig principles, such as advocacy of the

Union and belief in an orderly, hierarchical society in which an elite should rule. In so doing, she follows the excellent authority of Thomas B. Alexander, C. Vann Woodward, and other historians who have stressed persistent Whiggery in the postwar South. She does not seem aware, however, that recent research in the pre-Civil War period by Grady McWhiney, Lee Benson, Glyndon Van Deusen, and many others has seriously challenged her thesis that Whigs differed basically from Democrats in wealth, social position, and ideology. Since the basic presupposition of her book is currently being undermined, the author might have been well advised to minimize Whiggery as Alcorn's cardinal principle and to stress instead his consistent advocacy of the interests of the Mississippi Delta area, as opposed to those of the hill country. Indeed it seems likely that persistent patterns of intra-state sectionalism, largely the same patterns examined by F. J. Turner for the ante bellum period and by V. O. Key, Jr., for the twentieth century, were more important in shaping southern politics during the Reconstruction era than ideology, class, or even race.

Johns Hopkins University

DAVID DONALD

THE MARIETTA AND CINCINNATI RAILROAD, 1845-1883: A CASE STUDY IN AMERICAN RAILROAD ECONOMICS. By *John Pixton*. [The Pennsylvania State University Studies, Number 17.] (University Park: Pennsylvania State University. 1966. Pp. 94. \$1.00.)

THE railroad across southern Ohio, which is the subject of this brief monograph, differs from many of its contemporaries in having been the product of the efforts of small-scale local entrepreneurs. William P. Cutler, a prominent Ohio politician and promoter, and his rather shadowy associates knew little about railroading, but were powered by an almost invincible optimism concerning their eventual success. The jockeying among communities over the route, the problem of securing adequate capital, the persistent struggle to secure an effective eastern rail connection so that their road might gather in the largely illusory profits, it turned out, of through traffic to and from Cincinnati—these are the principal themes the author stresses in constructing his account. A final chapter deals with the Marietta and Cincinnati after it passed under control of the Baltimore and Ohio in 1868 and through its second receivership and reorganization in 1883. Manuscript materials of the Cutler family, including the diaries of William P. Cutler, and printed railroad records supply the main sources that the author has used.

This book is at its most revealing with respect to the railroad's financial history. Perhaps most surprising is the extent to which Cutler and his associates were able to draw upon English capital during the early years for what must surely have been an obscure railroad in English eyes. Probably because of the limitations of his sources, the author offers much less on problems of construction, administration, and operations. The narrative is at times cluttered with biographical material lightly related, if at all, to Cutler's railroad experience and is poorly proofread. It is supported by a useful map and a bibliographical essay.

San Francisco State College

GERALD T. WHITE

HOOSIER DISCIPLES: A COMPREHENSIVE HISTORY OF THE CHRISTIAN CHURCHES (DISCIPLES OF CHRIST) IN INDIANA. By *Henry K. Shaw*. (n.p.: Bethany Press for the Association of Christian Churches in Indiana. 1966. Pp. 535. \$7.50.)

Dr. Shaw affirms in his foreword: "Two categories of readers were kept in mind in the preparation of this book. They are laymen who may discover inspiration and develop appreciation for their Disciples' heritage and scholars who are seeking factual information." Loyal Disciples laymen, denominational scholars, and students of Indiana history will agree that the author's reach has not exceeded his grasp. The rest of us, perhaps, will regret that Shaw did not choose to extend his reach, for the result is rather more informative than illuminating, descriptive than analytical, factual than interpretive, and, in appeal, limited than broad.

Though an ordained Disciples minister, Shaw writes as a historian rather than as a pamphleteer, and the product is sound, if narrow, history. For one thing, he is judicious in weighing evidence (particularly church statistics, notoriously unreliable), and content to leave some questions unanswered when the evidence is too fragmentary or contradictory to permit final answers. For another, he can be abrasive in his judgments of his people, and, indeed, occasionally permits himself a touch of irony: "Though the Disciples knew very little about higher criticism, they were sure they were against it." Despite much interest in education, anti-intellectualism was a pervasive flaw among Hoosier Disciples. For a third, his research in primary sources is thorough, especially in the files of obscure religious periodicals. The bibliography, however, is either trimmed too closely or the author neglected a number of monographs relevant to his story. If the former is the case, it is difficult to explain the inclusion of such items as *Reader's Digest* with no further identification of articles or issues; if the latter is true, it helps to explain why the author loses his sure touch when dealing with broader national themes. Fourth, I caught no errors of fact, though several of interpretation, and very few typographical slips. Finally, the spirit of the book is irenic, and Shaw (appropriate to his religion) is wonderfully fair to competing faiths.

Here, then, is a narrative of the origins of a religious movement and its development into a major and respected denomination as traced chronologically in one state. The origins of the sect are well delineated, as are the tensions created in the evolution from sect to church. Less successful is the treatment of the relationship between the Indiana Disciples and such national movements as the social gospel, prohibition, and the Ku Klux Klan. The volume may be read with considerable profit by students of American church history, but only Disciples or Hoosiers will find the experience pleasurable.

University of North Carolina

ROBERT MOATS MILLER

SWORD OF PESTILENCE: THE NEW ORLEANS YELLOW FEVER EPIDEMIC OF 1853. By *John Duffy*. (Baton Rouge: Louisiana State University Press. 1966. Pp. x, 191. \$5.00.)

THIS work presents a detailed history of the yellow fever epidemic that struck

New Orleans in 1853. The author's purpose is to show how the government, the citizens, and the physicians reacted and the effect of the experience on medical theories, the public health movement, and the city's development.

The epidemic was one of the most severe that the country has ever experienced in rates of sickness and death. One feels that it must have been a fearful experience for those who lived through it. In his careful review of events as they unfolded, Professor Duffy makes clear the extreme reluctance of both newspapers and civic authorities to admit the existence of the epidemic and the total incapacity of the city government before, during, and after the visitation to meet the needs of an expanding urban community for routine sanitation and other preventive measures or even for rudimentary palliative emergency action. The city council simply abdicated its responsibilities and left the job of caring for the sick poor to a voluntary group of philanthropic citizens. Through it all the citizens apparently remained remarkably calm while the doctors, true to the ideals of their profession, courageously stood by their patients, however much they disagreed among themselves as to proper modes of therapy.

Duffy's discussion of the relative significance of this epidemic in speeding long-term trends in medicine and public health remains conjectural. His sources, chiefly newspapers, medical articles, and proceedings of the city council, are used with care, but one may wish he had made greater use of unpublished correspondence, diaries, and other sources that might have conveyed a more immediate impression of individuals' reactions to the terror: the horror is apprehended rather than felt. The account of the epidemic itself is competent and thorough and adds another welcome examination in depth of Americans' reactions to disease.

National Library of Medicine

JOHN B. BLAKE

THOMAS MORAN: ARTIST OF THE MOUNTAINS. By *Thurman Wilkins*.
(Norman: University of Oklahoma Press. 1966. Pp. xvi, 315. \$7.95.)

EVERYONE who has visited Grand Teton National Park knows the name Moran—Mount Moran. Not so many know that Moran was an artist, not a mountain man. And so fleeting is fame that hardly anyone is likely to know of Thomas Moran, influenced by Turner, admired by Ruskin, whose seventy-one-year artistic career which began in 1853 included water color and book illustration and etching, as well as the oils for which he was most famous. This, the first published biography of Moran, should revive general interest in the artist, renew appreciation of his work, and place him properly in the pantheon of American landscape painters.

Moran lived longer and painted more than most artists whose names are associated with the American West. Indeed, for his reputation and the price of his canvases, he seems to have lived too long and painted too much. After mid-career he was neither emulated nor much collected. This may be due less to changes in methods or "schools," as the author believes, and more to the artist's own weaknesses. Moran was attracted only by scenery. He judged everything by its "picturesque" quotient. That a painting might convey an abstract idea seems not to have occurred to him. When American collectors had been sufficiently convinced,

largely by Moran, that their country had scenery as grand as anything in Europe, Moran's works commanded less attention.

But as a depicter of the picturesque, the grand, the immense (some said the unpaintable), Moran had no equal in America. His paintings of the Colorado Rockies, the Grand Canyon (surely his favorite), the Southwest, and even more placid eastern scenes are unsurpassed in execution, and, more important to history, in their influence. His western landscapes were most effective as impetus toward national park legislation, hence, Mount Moran. This was the artist's great contribution and the one most stressed by the biographer.

To write a biography of Moran must have been frustrating. The artist left few personal records other than his art. Many of his most important excursions can be documented only by close consideration of sketches or finished work. Thurman Wilkins deserves special credit for careful and imaginative use of sometimes fragmentary sources. But the portrait that emerges seems a bit flat, a bit like one of Moran's own landscapes, lacking humanity.

The book is produced in the usual superior style of the University of Oklahoma Press and includes forty black-and-white and eight color plates.

University of Oregon

MARTIN SCHMITT

THE CAPTAIN'S BEST MATE: THE JOURNAL OF MARY CHIPMAN LAWRENCE ON THE WHALER *ADDISON*, 1856-1860. Edited by *Stan-ton Garner*. [Brown University Bicentennial Publication.] (Providence, R. I.: Brown University Press. 1966. Pp. xxi, 311. \$8.50.)

In the later days of whaling it was not uncommon for a captain's wife to accompany her husband on a long voyage. This is exactly what Mary Lawrence did in 1856-1860. Furthermore, she had their small daughter along. Her husband, Captain Samuel Lawrence, and his five brothers of Falmouth, Massachusetts, were all whalers. She was the strong, though physically small, and somewhat pious New England helpmate typical of the time and calling.

The voyage of the *Addison* is divided into seven cruises: the first from New Bedford to the Sandwich Islands; the next five, based on those islands, took the ship to Alaska, the South Pacific and the Marquesas Islands, the Arctic, Lower California, and to Alaska and the Arctic once more; the last cruise was the voyage home via New Zealand. It was not a particularly successful voyage. Whales were scarce.

Whaling was a rough, callous business, and yet Mary Lawrence's journal is not much different than if she had been crocheting in her own living room in Falmouth. Her interests are those of a good housewife: food, chores, visiting with other captains and their wives, and speculating over what neighbor or relative might be aboard the next sail seen on the horizon. She mentions when whales are taken, but there are no descriptions of the chase.

Obviously the captain ruled the ship with a hand of iron. If any vulgarity reached her ears, it was ignored. On the whole this is a rather amazing, subdued chronicle that shows how boring life at sea could be on the usual unadventurous voyage.

The whaling historian will value the great numbers of vessels mentioned, giving exact dates, their masters, what they were doing, and how they fared. An informative and attractive feature of this well-made book is the map of each cruise to help the readers' orientation.

Peabody Museum of Salem

ERNEST S. DODGE

THE PETITIONERS: THE STORY OF THE SUPREME COURT OF THE UNITED STATES AND THE NEGRO. By *Loren Miller*. (New York: Pantheon Books. 1966. Pp. xv, 461. \$8.95.)

THERE are few novelties in this study of cases and social developments governing Negro and white relations. Events, justices, partisans are well known, and the cases on which they joined issue have, in many instances, almost classic stature, involving as they do rights before the law, segregation, housing, education, and public privileges. The author makes the difference. Milton R. Konvitz' *A Century of Civil Rights* (1961) is an excellent handbook, but it lacks all the variety, the flavor, and the style that characterize the present work. Mr. Miller himself is a judge of the Los Angeles Municipal Court who has long been connected with NAACP efforts to modify legal restrictions on Negro hopes and opportunities.

He cites a lawyer, in the case of his own granduncle, in 1875, who was "properly shocked" by certain legal interpretations, "as lawyers always profess to be when their opponents express a view contrary to the posture they have assumed as an advocate of a cause." He, too, however, does not hesitate to be "amazed" or otherwise emotional in his responses to historical developments. But he works from sound materials, including court records, and varied, relevant writings of commentators and historians. He writes from a combination of shrewdness and principle, as in his appreciation that Jefferson, in his Declaration of Independence, wrote as a propagandist but also as a man of ideals and good hopes. Miller's own attitude toward the Emancipation Proclamation takes the now fairly standardized liberal interpretation respecting content and intention. Miller is realistic; he is conscious of the fact that judicial review often acted to favor the slaveholder and opponent of expanded rights for Negroes, but that it also, on occasion, weighed on the libertarian side and that the key to change lay not only in the Supreme Court, but elsewhere, as in the abolitionist crusade or, later, in the Reconstructionist Congress.

Miller himself is, I am sure, too sophisticated to imagine that his approach will be shared by the Virginia Commission on Constitutional Government, and he need not fear that his surprise, irony, and indignation will not stir sympathy in some of his readers. It will also augment their information. The historian proper would be wise to temper his appreciation of Miller's account with qualifying suggestions. The author spices his narrative with touching phrases and anticipations. "Old Ark A-moverin'" indicates changing climates of opinion. "Wait a Little While," "Tall Like Free Men," and "Bright Before Us" define his judgment of changing legal conditions. But Miller, especially in his Los Angeles milieu, must know that walls do not so much tumble down as change their shape and other qualities. The

historian with something to contribute to history will avoid à la mode gestures in inappropriate places, which look merely quaint or opportunistic as time passes.

Antioch College

LOUIS FILLER

HINTON ROWAN HELPER: ABOLITIONIST-RACIST. By *Hugh C. Bailey*.
[Southern Historical Publications, Number 7.] (University: University of Alabama Press. 1965. Pp. xi, 241. \$6.95.)

At first glance it would be hard to imagine a more marginal type, or a man whose ideas, loyalties, and prejudices were a greater tissue of contradictions, than Hinton R. Helper. Helper was a southerner who hated slavery, a nationalist who breathed class warfare, and an abolitionist who despised the Negro. Fascinated by statistics and, indeed, viewed by some of his contemporaries as a man driven mad by figures, he devoted most of his life to romantic schemes.

Yet as Professor Bailey points out in his careful study of Helper's career, it is the rigidity of our own social and intellectual categories that keeps us from recognizing Helper as a fully plausible nineteenth-century type. It was just that Helper's timing was a bit off. With *The Impending Crisis*, he got a national reputation for bitterly attacking slavery, and the planting aristocracy that slavery supported, at a time when De Bow and others were arguing that all southern white men had a fundamental interest in maintaining the system whether they were slaveholders or not. Helper had the worst of the argument, even with the small farmers for whom he was speaking. From the southern viewpoint in the year 1857, his book was eccentric in all the wrong ways, and the statistical tables that he used as his chief weapon seemed willfully perverse.

But the closer one looks at Helper, the clearer it is that he was in fact a true intellectual spokesman for his class. Why, then, did the yeoman farmer element not recognize him as one of their own? He showed them plainly enough what the planter aristocracy, bulwarked by its slave labor force, was doing to them. By pre-empting the best lands, systematically skinning them, and forcing the entire region along the narrow track of a staple-crop economy, the planter class inhibited the economic development of the South and choked off the avenues to opportunity for everyone else. The small farmer was thus condemned to poverty. Helper had his story fairly straight. His use of statistics may have been occasionally questionable, but the most recent studies on the ante bellum economy show that his main argument was not essentially wrong.

Furthermore, his bitter racism was the common heritage of yeomen everywhere. He wanted emancipation, but he also wanted, just as they would a few years later, to eject every Negro physically from the country. But, again, the trouble was his timing. He deliberately de-emphasized his schemes for forcible exile because they would alienate the only force then prepared to support him in emancipation: the Republican party. Only in the North could his book even be published; it was thus he who had to become the exile. Like Andrew Johnson, Helper was soon sickened by the Republicans' shift to radicalism after 1866. Universal suffrage and equal rights were the last things he had in mind for the Negro when he argued for immediate emancipation in 1857, and the Negrophobia

he spewed forth in all his subsequent writings was an accurate reflection of what his feelings had been all along.

With regard to Helper's solution to the Negro question, only chronologically was his thought much off the main line: somewhat behind the North, a little ahead of the South.

Smith College

STANLEY M. ELKINS

DEAR ONES AT HOME: LETTERS FROM CONTRABAND CAMPS.

Selected and edited by *Henry L. Swint*. (Nashville, Tenn.: Vanderbilt University Press. 1966. Pp. 274. \$6.95.)

Two Quaker sisters, Lucy and Sarah Chase, who left their New England home in 1863 to teach and work with freedmen within the Union lines in the South wrote most of these letters, though a small number of those included are addressed to them. The manuscripts constitute one of the richest collections of private letters written by northern teachers working in the southern states during and after the Civil War.

Lucy was the more communicative of the sisters, writing at great length and with much detail about life among the refugees in Craney Island, mainland Virginia, and the Deep South where the sisters worked. The letters included observations on the manners, morals, and dialect of the freedmen, descriptions of Richmond, Charleston, and Savannah, comments on the difficulties encountered by New England schoolmarm laboring under wartime conditions in occupied territory, and plentiful testimony on the rude and sometimes arrogant attitude of southerners with whom they came in contact.

There is a wealth of source material in this well-edited book. Despite the markedly paternalistic attitude that colored much that the Chase sisters wrote, their letters add considerable detail to our knowledge of life in the contraband communities. Lucy told the story of one former slave who had been parted from her husband as a young woman and who remarked: "White folk's got a heap to answer for the way they've done to colored folks! So much they won't never *pray* it away!" The years of service volunteered by Lucy and Sarah Chase and thousands like them made slight atonement for that "heap," and it is important to have the record preserved and made available as it is in this volume.

Wilmington College

LARRY GARA

THE FARMERS' FRONTIER, 1865-1900. By *Gilbert C. Fite*. [Histories of the American Frontier.] (New York: Holt, Rinehart and Winston. 1966. Pp. xiv, 272. \$5.95.)

This fourth volume in a projected eighteen-volume series is one of seven that will deal with aspects of the frontier story after 1860. The editor says that this book "is about the farmers who won America's last frontier and by so doing wrote a saga of epic proportions." In his preface Professor Fite says that the "main reason" for writing this account "is that there is no book that covers the subject" and that "the history of agricultural settlement is highly important because farm-

ers, more than any other group, were mainly responsible for finally bringing the frontier to an end." Later he states that "this book is more a history of agriculture than of farmers or farm life." Apparently editor and author were not in complete accord.

Between 1860 and 1900 American farming experienced great growth and change. The number of farms increased from 2,000,000 to 5,700,000; the principal source of power for farming began to shift from men and animals to steam and internal-combustion engines. Railroads in 1860 had barely extended beyond the Mississippi; by 1900 they served the vast landlocked region west of the Missouri. In 1860 there were only a few agricultural colleges; by 1900 over fifty had come into existence, and some had made discoveries of fundamental importance to the agricultural revolution. In this book Fite is concerned only with that part of the expansion of farming that came from advance into the new lands west of a line from St. Paul to Ft. Worth. (Fite excludes western Iowa from consideration with the ambiguous explanation that it was "much less of a pioneering area than Minnesota" and areas further west.) About half of the book is devoted to creating farms in Minnesota, Kansas, Nebraska, and the Dakotas. There are separate chapters on Oregon and Washington, California, the Rocky Mountain farming frontier (Here a chapter of seventeen pages discusses farm settlement in Montana, Idaho, Wyoming, Utah, Colorado, New Mexico, Arizona, and Nevada!), and west Texas, Indian Territory, and Oklahoma. The author has used primary source materials, but there is little in the monograph that alters what is already known. An alert copy editor could have improved the book although as it stands it provides a useful but sometimes myopic picture of the farmers' frontier after the Civil War.

University of Washington

VERNON CARSTENSEN

THE SMILING PHOENIX: SOUTHERN HUMOR FROM 1865 TO 1914.

By *Wade Hall*. (Gainesville: University of Florida Press. 1965. Pp. xv, 375. \$8.50.)

IN this voluminous study the author attempts to analyze a wide range of southern literature in the search for a vein of humor. At times the reader all but becomes lost in the labyrinth of authors and titles. Professor Hall introduces almost every southerner who ever picked up a pen to write a novel, short story, or editorial. The anatomy of humor that appears in so inclusive a study becomes dry boned in places. The distillate of this study is not so much the essence of humor as the spiritual and emotional struggle of post-Civil War southerners to fit themselves into a new national society with defeat against them on one hand and the rise of industrialism on the other. They attempted to do this without surrendering their regional character.

The author deals at length with the significance of southern humor in attempts to bring about a spiritual reunification of northern and southern people, and of the nation. The rejoining of the social union involved a tremendous amount of self-consciousness on the part of both northerners and southerners. Romances that blossomed during the Civil War necessitated much forgiving and

forgetting. Southerners, like people facing adversities everywhere, were able to conjure up enough wit to ease their way into the future. The local colorists, dialecticians, editors, short-story writers, and yarn spinners drew heavily from the southern resource of poor whites, newly freed Negroes, and the rank and file of ladies and colonels. The former slave also lingered long in the South in body and spirit to inspire southern authors.

The author pulls a long bow of political and social judgment when he contends that, "By the 1890's the South had been so successful in promulgating this image [Uncle Remus, Uncle Si] that relatively little opposition was heard when southern lawmakers accelerated their disfranchisement of the Negro." He believes this placed the South in the same position from which Lincoln's forces had routed it in the 1860's. He speculates whether incidents such as those in Jackson, Little Rock, and Birmingham would actually have occurred if southern literature had taken a different slant. This much seems certain: there was no humor in these situations.

Hall all but drowns his subject with details. One gets the impression that all southerners and literature have been larded with humor. What of hate and violence? There is an extensive bibliography included in both the text and the formal listing of sources.

University of Kentucky

THOMAS D. CLARK

MISS MARY CASSATT: IMPRESSIONIST FROM PENNSYLVANIA. By Frederick A. Sweet. (Norman: University of Oklahoma Press. 1966. Pp. xx, 242. \$7.95.)

In the history of American culture a curious fate has overtaken Mary Cassatt. While her participation in French impressionism gives her a place in books on that subject, she does not appear at all in several standard histories of American art. Literary historians do not seem to consider his life as an expatriate a reason for excluding Henry James from American literature, but art historians have apparently been more parochial. This first full-scale biography of Cassatt should be a corrective: Mr. Sweet puts her squarely back in American culture. He is primarily concerned with her development as an artist, but he sees her talents emerging in the complex social setting of an expatriate, both supported and burdened by expatriate parents. His study is particularly based on unpublished letters, especially those exchanged within the Cassatt family. Since Mary was the sister of Alexander, president of the Pennsylvania Railroad, this is a useful work for social historians as well as those concerned with the history of painting.

Sweet presents an extraordinarily independent woman who went to Paris to study art in her early twenties (unusual even for the relatively emancipated American woman of the late nineteenth century) and returned only for rare and brief visits. She became an associate of the *avant-garde* in the Paris art world while at the same time maintaining social standards current in an upper-middle-class American family of the period. Recognition as a professional painter, let alone a distinguished one, was slow in an America unfamiliar with Impression-

ism and loathe to grant professional status to a "lady painter." Her first visit here in twenty years was reported by the Philadelphia *Ledger* in 1899: "Mary Cassatt, sister of Mr. Cassatt, president of the Pennsylvania Railroad, returned from Europe yesterday. She has been studying painting in France and owns the smallest Pekingese dog in the world."

Because of her unique position as an American artist living abroad with extensive connections among wealthy American families, "she exercised a stronger influence on art collecting in America than any other individual of her day." She was also instrumental in organizing the Durand-Ruel exhibition in 1886, the first major showing of Impressionist painting in America. One is grateful to Sweet for restoring Cassatt to American culture, but he could have given more than tantalizing glimpses of the relationship between this aristocratic representative of American culture and her Bohemian French colleagues.

This is a work of meticulous scholarship, although the lack of footnote references is somewhat disconcerting. The book is handsome and well illustrated.

Finch College

RUTH MILLER ELSON

POLITICS AND POWER: THE UNITED STATES SENATE, 1869-1901. By David J. Rothman. [Publication of the Center for the Study of the History of Liberty in America, Harvard University.] (Cambridge, Mass.: Harvard University Press. 1966. Pp. x, 348. \$6.95.)

THE Gilded Age in American politics has been kicked and scuffed among historians until there is little left of its reputation. Most historians believe that at no other period in American history was the moral and intellectual tone of political life so uniformly low nor were political contests so preoccupied with patronage.

Professor Rothman is not defending the political record of the Gilded Age so much as he is re-examining the "dubious interpretations" and "unfounded accusations" of the period. Discovering that recent studies of politics for the years 1869-1901 offered few insights into the structure of party and power in the country and "astonished at the extent to which Mugwump-Populist-Progressive attitudes have been entrenched in the texts," Rothman decided an institutional account of post-Civil War politics might be rewarding.

He chose the United States Senate as the most appropriate institution for the study because it enjoyed pre-eminence in the government, was where most major pieces of legislation took final form, and, even more important, assumed its modern form in the post-Civil War period. This alteration in the Senate, its causes and implications, is the theme of this book. Party leadership and party machinery took on their contemporary procedures and significance. By 1900 the party caucus and its leaders in the Senate determined the committees and the business calendar, and the parties exercised key powers over the chamber's proceedings. In seeking an answer as to why the Senate was so changed in these years, Rothman discovered that by 1900 senators had fundamental attitudes markedly different from their predecessors. They came to the Senate ready to accommodate and promote party organization, and, because of this, they helped change the character of the upper

house. Finally, Rothman disputes the Populist-Progressive view that senators were "in the pay of the corporation," and, contrary to many popular beliefs, the Senate did not invariably reward highly immoral, if not illegal, methods. Party men, not businessmen, comprised the only real community in Washington. The post-Civil War years did not represent the acme of corruption; rather they witnessed the start of the modern system by which pressure groups vied for government support.

Rothman has made a careful analysis of his subject and has written an important and stimulating book, based largely on manuscript sources. His industry in this matter is to be commended, for he and his wife went through practically every known collection of senators' papers for the late nineteenth century. Rothman's reinterpretation will, I am sure, stimulate others to test his findings as he has tested those of Josephson, Beard, and others.

University of Notre Dame

VINCENT P. DE SANTIS

IRISH-AMERICAN NATIONALISM, 1870-1890. By *Thomas M. Brown*. [Critical Periods of History.] (Philadelphia: J. B. Lippincott Company. 1966. Pp. xvii, 206. \$3.95.)

THIS remarkably perceptive book is a fine example of recent attempts to raise the study of Irish-American history above the level of filiopietistic reminiscence. Professor Brown succeeds admirably and concludes with a masterful bibliographical essay that will benefit students of American and Irish history. Encompassing the most thoroughly researched period of Irish history, he provides a superb synthesis of the history of Home Rule and the Land League. In focusing, however, on the American Irish, he emphasizes, in contrast to Daniel Moynihan, the importance of the American milieu in shaping Irish-American political behavior. In comparison with Allan Nevins' biography of Grover Cleveland and Florence Gibson's study of the New York Irish, moreover, Brown's book is more sympathetic to the peculiar needs of Irish-Americans in the Gilded Age.

According to Brown, there were two forces in Irish-American history: Irish nationalism and the search for social justice. The former was the more powerful and brought into the American branch of the Home Rule movement both radical reformers and conservative businessmen. During the 1880's, when Irish-Americans developed into a political force in the United States, they abandoned the ethical and social concerns of Henry George and Michael Davitt. Indeed, they preferred the charisma of Parnell to the Christianity of Davitt.

The Irish became the great pragmatists of American politics. They were able to build the structure of modern urban politics because of their concentrated numbers, their techniques of organization learned from O'Connell, and their driving desire for power. Similarly, they provided leadership for the American Catholic Church. Adjusting to their American environment, they became worshipers of "the bitch goddess success" and sought power for its own sake.

Parvenu aspirations, however, were at war with the spirit of nationalism. After the Home Rule fight of 1886, the Irish-American power structure, in its search for identity, shrilly emphasized Irish solidarity, but found the task increasingly diffi-

cult. Irish-American nationalism narrowed and became more intensely Philistine and puritanical. Social discrimination and a sense of isolation created a Gaelic power bloc and the development of a narcissistic nationalism. Self-pity was the cement of the power bloc but also the inspiration for the caricatures of the professional Irishman. Irish-American solidarity, however, like Parnell's career, was shattered by *l'affaire* O'Shea in 1890, and the disillusioned nationalists turned on each other.

Catholic University of America

JOSEPH M. HERNON, JR.

INDIAN POLICE AND JUDGES: EXPERIMENTS IN ACCULTURATION AND CONTROL. By *William T. Hagan*. [Yale Western Americana Series, Number 13.] (New Haven, Conn.: Yale University Press. 1966. Pp. viii, 194, 12 plates. \$6.50.)

THIS book deals with one aspect of the complex problem of assimilating American Indians. When proud warriors became incapable of resistance to frontier advancement, they were compelled to reside on western reservations where missionary teachers offered religious, moral, academic, and vocational instruction. Essential to the assimilation program was some system of law and order.

Professor Hagan has illustrated the function of Indian policemen in making the authority of agents effective without the assistance of military officers and troops. He has not shown, however, that the Indian police system grew out of the "Peace Policy" inaugurated under Grant, which gave the army authority over Indians outside reservations and intended that civilian agents should have independent jurisdiction within the reserves.

Courts of Indian Offenses, made up of Indian judges, were an extralegal expedient explained by the failure of Congress to substitute federal judicial machinery for tribal authority, which the government ceased to recognize in 1871. Congressional misgivings about constitutional grounds for governing reservation Indians and contact with white people, which diminished the power of chiefs, led to the establishing of such courts because of urgency. As in the case of the police, the author missed an opportunity to develop this background. He has succeeded, however, in probing both the strengths and the weaknesses of the courts and police.

Whether dealing with police or courts, Hagan is inclined to view them primarily as means of imposing a foreign control and culture upon Indians. While such a judgment has merit, it ought to be tempered by recognition that tribal control was badly disrupted before police and courts were established.

The results of intensive research in federal records and at the Oklahoma Historical Society are exhibited throughout this volume. By incorporating new material with that found in printed sources, the author has made an important contribution to understanding the difficulties of Indian-white coalescence. It is unfortunate that the text is marred by several defects that a diligent editor might have eliminated.

St. Olaf College

HENRY E. FRITZ

INDIAN FIGHTS: NEW FACTS ON SEVEN ENCOUNTERS. By J. W. Vaughn. (Norman: University of Oklahoma Press, 1966. Pp. xv, 250. \$4.95.)

BECAUSE it consists of seven essays on various topics connected with the Indian wars on the Great Plains, this volume is somewhat difficult to evaluate and review. Mr. Vaughn has long been interested in these wars and has a number of books on various phases of the subject to his credit. These studies, for the most part, represent material that he was unable to use in his previous books or that has only recently come to his attention. One or two of the essays are revised reprints.

The articles themselves do not fulfill the promise of the subtitle, and they are of varied interest as well as being somewhat uneven in quality. To me, the most interesting chapter was that dealing with Major Reno's lines in the valley, an engagement that constituted the opening phase of the Battle of the Little Big Horn. Vaughn seems to have settled that subject, at least as far as it can now be settled, once and for all. But the article itself adds little to the excellent map, except that the author takes the reader behind the scenes of his research and shows him how the conclusions were arrived at, together with some other interesting and pertinent details.

The chapter "A New Look at the Fetterman Disaster" suggests that Captain Fetterman, instead of disobeying orders, may have acted in accordance with what he believed Colonel Carrington actually wanted him to do, and, since "dead men tell no tales," he was made the "goat" of the affair after its tragic conclusion, which is hardly a new concept. Vaughn also attempts to work out a chronology or sequence of details for the destruction of Fetterman's command, and in this he handles his evidence somewhat strangely. On page 77 he not only accepts Cheyenne accounts as to the sequence of events of the fight, but bases his argument on what they said; two pages later he rejects other Cheyenne testimony on the subject as being unlikely.

Space limitations make any further detailed consideration impossible. There are a number of maps and some well-chosen illustrations. The bibliography is restricted to major sources only, and there is an adequate index. All in all, it is not too much to say that most western history buffs will find all of the essays worth reading, besides discovering much that is of particular interest.

Eastern Washington State College

EDGAR I. STEWART

BANKERS AND CATTLEMEN. By Gene M. Gressley. (New York: Alfred A. Knopf, 1966. Pp. xix, 320, viii. \$6.95.)

THE first chapter of *Bankers and Cattlemen* is a rather uninspiring, introductory account of some leading Wall Street financiers who invested in the cattle business, as seen through the eyes of one enjoying a business fellowship at Harvard. Fortunately, the academic influence wanes after the first chapter, and the balance of the book is a fresh account of the role of eastern capital in the cattle industry. It is written in the best western tradition with humor, subtlety, freedom from clichés, and good balance. The author has spiced his story with choice bits drawn from the many manuscript collections relating to the cattle industry at the Western History

Research Center of the University of Wyoming, from numerous local newspapers and diaries, and from recollections, published and unpublished.

The period 1870-1890 was an era when venture capital was investing in the rapidly developing Far West. Railroads were pushing into hitherto remote and nearly inaccessible areas, population was flowing westward in a mighty stream, land values were breaking through previous highs, and cattle ranching, along with mining, lumbering, and city building, was offering spectacular profits. Readers may well conclude that the bankers were not solely attracted by stories of large profits in the cattle industry, but were willing and indeed anxious to have a part in the development of the last great west. Perhaps even they were caught up in the drama of that development that has, unfortunately, been so distorted by popular media in a later generation.

With a judicious mixture of facts, amusing incidents, and stories, all carefully documented and properly evaluated, the author delves deeply into the financial history of the cattle industry. The methods of attracting eastern capital, the structure of the companies, and the role of the manager and of the commission firm are examined. The optimism of the early correspondence turns to sheer pessimism as droughts, overstocking of the range, overproduction, and declining prices reduced the prospects of profit and forced the elimination or cutting of dividends.

Previous studies of the cattle industry have emphasized the ranchers' optimistic tendency to overrate the numbers of their cattle, the harsh winters, and the heavy losses of stock revealed at the spring roundups. The author, who is concerned to produce a business history, does not give much space to these topics. He gives less attention also to foreign capital investments—a topic already well treated by Turrentine Jackson. Possibly his discussion of the dread scourge, pleuropneumonia, and the part it played in bringing about the creation of the Bureau of Animal Industry should also take into account the heavy losses inflicted on the dairy industry by this disease. Railroad rates seemed fair to the author, at least in so far as they affected the livestock industry.

Some critics may find that Gressley reflects more the viewpoint of the cattlemen than of the homesteaders; certainly there is a great difference between his brief mention of the Johnson County War and the treatment given it by Mari Sandoz in her classic *Cattlemen*. My own judgment is that the author, in sticking to his topic, has produced a fine study of the role of eastern capital in the cattle industry.

Cornell University

PAUL W. GATES

THE COMPANY TOWN IN THE AMERICAN WEST. By *James B. Allen*.
(Norman: University of Oklahoma Press. 1966. Pp. xvii, 205. \$5.95.)

THIS valuable study of about two hundred identifiable company-owned towns in the eleven western states concludes that the chief characteristic was diversity. In illustrating this diversity, Dr. Allen has a chapter each on lumber towns, copper towns, coal towns, and such other company towns as those connected with the mining and manufacture of molybdenum, potash, cement, and petroleum, and with the growing of cotton. The author then switches to a topical arrangement, with chapters on the management of company towns, community planning and

housing, human welfare, the company store, and political and economic paternalism. An appendix provides brief histories of each of the towns identified. The chief sources of information were personal visits and interviews, company papers, newspaper stories, government reports, and masters' theses and doctoral dissertations of particular enterprises.

Although he finds the "worst" paternalism and exploitation in coal mining towns around the turn of the century, Allen concludes that the negative image usually attached to company towns has been considerably overplayed. While they offered undeniable opportunities for abuse, company towns were a necessary means of attracting workers to the remote sections in which mining and lumbering operations had to be conducted. It would be interesting to know whether a more thorough search of labor union literature would confirm the rather favorable image that Allen presents. More specifically, Allen does not make much of the possibility that religion may have been used as a discipline, although priests and ministers in company towns were usually antiunion, or, at least, not anticompany. Allen believes that the antilabor image was truer of company towns in the East than of those in the West, owing to the greater shortage of labor in the West and the necessity of giving labor better treatment. Western workers, moreover, probably were capable of a higher degree of mobility than workers east of the Mississippi.

With improvement of transportation facilities, companies now find it possible to use workers who live in residential districts that are more desirable than those provided by the company. Company towns are fast becoming "history"; many have been sold to middlemen who have sold them in turn to individual workers and businessmen. One exception to this is the federal government village that has multiplied around defense centers during World War II and since, and around such isolated enterprises as the Flaming Gorge and Glen Canyon Dams. It would be interesting for Allen to contrast the federal town with the company town.

This monograph represents an important contribution to American business and industrial history and to the local history of many western communities.

University of California, Los Angeles

LEONARD J. ARRINGTON

THE GENTLE REFORMERS: MASSACHUSETTS DEMOCRATS IN THE CLEVELAND ERA. By *Geoffrey Blodgett*. (Cambridge, Mass.: Harvard University Press. 1966. Pp. xiii, 342. \$6.95.)

Most people in political life found it easy to dislike mugwumps. Theodore Roosevelt called them "political and literary hermaphrodites," and the *Republic of Boston*, an Irish newspaper, referred to them as "'independent cranks,' professing to be reformers." When Massachusetts mugwumps moved into the Democratic party in 1884, the welcome they received was less than enthusiastic. Only the hope of electing Cleveland dictated a welcome of some kind. The Massachusetts party had been an odd mixture of Irish and conservative Yankees, many of whom had important commercial interests. The addition of mugwumps made the balance exceedingly precarious, but the Massachusetts Democrats held together for twelve years, established a respectable record in elections, and enacted a much-needed

but on the whole cautious reform program in the state. The depression of the 1890's and the Bryan campaign wrecked the Massachusetts Democracy. The mugwumps and their civil service reformism became irrelevant (or more irrelevant) when unemployment became extremely serious, western farmers put forward radical demands, and railroad strikers encountered the armed force of the Cleveland administration.

Professor Blodgett has organized his difficult story well and has told it with clarity and occasional grace. He is particularly interesting on the mugwumps, perhaps because these highly articulate political dilettanti left more of a paper record than their less intellectual colleagues did. The author displays real flair for biography in his several sketches of largely forgotten Bay State Democrats, some of whom, such as Governor William Eustis Russell, Mayor Josiah Quincy of Boston, and Congressman George Fred Williams, deserve to be known better. The color of these men and of such Irish leaders as Patrick A. Collins and Patrick Maguire lends a refreshing respite from the account of the bland activities of the gentler reformers. Blodgett researched an impressive number of manuscript collections and newspapers, nearly all of them concerning the Boston area. About 40 per cent of the Massachusetts population in 1890 lived within twelve miles of the state-house, and it was probably the most important 40 per cent. Greater attention to the western part of the state might have been rewarding, but this is a minor flaw in a nicely executed monograph.

University of Maryland

DAVID A. SHANNON

THE AMERICAN 1890s: LIFE AND TIMES OF A LOST GENERATION.

By Larzer Ziff. (New York: Viking Press. 1966. Pp. viii, 376. \$7.50.)

THIS is literary history at its best. Mr. Ziff has carved out a decade, the ten years from the summer of 1891, when six thousand workers were busy preparing the site at Jackson Park for Chicago's Columbian Exposition, through 1900, the year of the publication of Dreiser's *Sister Carrie*, and, with apparent effortlessness, he has made sense out of one of the most confused and perplexing periods in American literary history.

He has done this by a skillful correlation of the political, social, and intellectual events and movements of the time with the forms and meanings of the work of its major writers, studied individually, and its minor writers, studied in related groups. There is nothing very radical about this process except that there seem to be so few literary scholars who can do it without getting carried away by cultural history or analytical criticism as ends in themselves. Ziff's hand on the tiller is sure at every point, and a book that starts as a somewhat obvious review of much that is already known ends by being a unified and enlightened chronicle and interpretation of the literary experience of a people during a given period in their history.

Our old friends are all here: "Realism," "Regionalism," "Progressivism," "Naturalism," and "Romanticism," among others, but they do not obtrude. They are, what they always ought to be, adjectives rather than nouns to qualify and help describe literary phenomena rather than to force them into a higher and largely

mythical unity. After a brief and entertaining chapter on the "Land of Contrasts," which sets his basic thesis—that this decade was really the first of the twentieth century rather than the last of the nineteenth—he proceeds to conclude that the literary giants who survived it did so because they were aware of "nothing of the turbulence of the nineteenth century's closing, nothing of the shift to the city, of the extension of the limits of sexuality in literature, of the changing social position of women, of a poetry of precision and felt experience, of the fall from the innocence of national isolation." Those who were aware of these forces succumbed by the end of the century or lapsed into at least temporary silence: Crane, Norris, Frederic, Kate Chopin, Robinson, and Dreiser.

There are minor errors, of course: the index is wholly inadequate; there are smart asides that would do better for the classroom than for the printed page; and there is an obvious indebtedness to his predecessors (Beer, Mumford, Cargill, Foerster, Curti, Commager, Hofstadter, and so forth), which is not always overtly acknowledged. These things can, however, be accepted as small cost for a clear and steady focus on the main issue, a remarkable skill in subtle and firm over-all organization, and a lively and mobile style. This is a book to read and remember.

University of Pennsylvania

ROBERT E. SPILLER

THE WAR ON POWDER RIVER. By *Helena Huntington Smith*. (New York: McGraw-Hill Book Company. 1966. Pp. xiii, 320. \$7.95.)

THE Johnson County, Wyoming, War of 1892 between large ranchers and homesteaders rivals Custer's "last stand," Billy the Kid and the Lincoln County War in New Mexico, and other perennial favorites among western buffs. Less bloody than some episodes, it had a particular color all its own, including the hanging of a woman during its preliminary phase and participation by Ivy League graduates belonging to prominent eastern families in an abortive effort to eliminate cattle rustling and encroachments upon the range. On April 5, 1892, fifty-two men, including cattlemen, stock detectives, hired gunfighters, and a reporter for the *Chicago Herald*, departed from Cheyenne by special train to carry out their mission of killing or driving out a marked list of individuals in the range country. They succeeded in killing only two men before they were besieged by Johnson County settlers and had to be rescued by federal troops. Money and influence saved them from punishment for their actions. Newspapers reported the episode in considerable detail but with less accuracy, and in 1894 Asa Mercer, Wyoming newspaperman, told the story in his highly partisan *Banditti of the Plains*.

Later accounts have ranged from sheer fiction to serious efforts to appraise the nature and meaning of the conflict. Smith's book definitely belongs in the latter category. She has searched out and critically examined widely scattered source materials and gives her reader a judicious appraisal of previous writing on the subject as well as her own conclusions. Although favorable to the homesteaders, she recognizes that the cattlemen had some cause for complaint. A measure of good and evil, or of indifference, could be found on both sides, and the rancher's greater arrogance came more from wealth and political influence than from innate defects of character. Smith has footnoted her material sufficiently to enable

the reader to check her sources and has avoided most of the clichés found in popular literature on the West. At the same time, she writes well, evoking a sense of time, place, and character that makes for interesting reading. Her book will appeal to the general reader and also to the scholar in proportion to the importance he may assign to the Johnson County War. It will certainly need to be consulted by anyone interested in the facts or the significance of that event.

University of Missouri, Columbia

LEWIS ATHERTON

MASTER OF PRECISION: HENRY M. LELAND. By *Mrs. Wilfred C. Leland* with *Minnie Dubbs Millbrook*. With an introduction by *Allan Nevins* and *Frank E. Hill*. (Detroit: Wayne State University Press. 1966. Pp. 296. \$7.95.)

HENRY Martyn Leland had a remarkable career. He had already achieved success as a designer and manufacturer of machine tools before he entered the automobile business at the age of sixty. He then gave to the automotive world both the Cadillac and the Lincoln. More important, Leland impressed on the early American automobile industry a standard of precision and accuracy, which, incorporated into the technique of mass production, made the difference between low-cost production and merely cheap production.

It is surprising that Leland has not found a biographer before, but it is gratifying that in his daughter-in-law he has found a good one. It would have been easy for Mrs. Leland and Miss Millbrook to indulge in uncritical adulation. It is clear enough where their sympathies lie, but they have made a point of getting expert advice, and they have produced a book stamped with professional competence. This is not only a biography based on family records, but also a useful and well-informed commentary on significant developments in the American automobile industry during Leland's lifetime.

Two features of the narrative are of special interest. One is the account of the development of the electric starter, and, in my judgment, Mrs. Leland has set the record straight. Her version can be corroborated from other sources: to wit, that the starting system was worked out in the Cadillac engineering department, and Kettering's contribution was limited to the motor. This involves no denigration of Kettering. The rest of the system would have been useless without a motor capable of turning the engine over and at the same time small enough to be put in a car, and there is generous acknowledgment of the importance of Kettering's battery-operated ignition, which, by using a battery instead of a magneto, made possible both electric starting and electric lighting.

The other salient feature is the story of the controversy between the Lelands, father and son, and Henry Ford over Ford's acquisition of the Lincoln Motor Car Company. It is admittedly an ex parte account of a tangled situation, but no reputable historian has been able to tell this story in a way that reflects much credit on Ford.

With so much of value in this book, it is perhaps quibbling to point out that the authors got lost when they ventured into aviation. Gnome and Hispano-Suizas were engines, not planes; De Havilland was a plane and not an engine.

Harvey Mudd College

JOHN B. RAE

LOUIS MARSHALL, DEFENDER OF JEWISH RIGHTS. By *Morton Rosenstock*. (Detroit: Wayne State University Press. 1965. Pp. 334. \$8.95.)

PUBLICATION of this book is an encouraging sign of the increasing interest trained historians are taking in American Jewish history. In Louis Marshall, Professor Rosenstock has found a subject worthy of study. Based on manuscripts and other primary sources, this account of Marshall's career is a significant addition to its field.

While this is not a biography in the usual sense since the author does not concern himself with personal details nor make any effort to "humanize" Marshall, the reader does gain a view of the whole man. Marshall, although of German-Jewish descent, realized before most of his social class that all Jews were concerned in the matter of Jewish rights and that they were also the subjects of unfavorable comments ostensibly aimed at recent immigrants. He was relentless in his denunciation of what he called the "cringing Ghetto spirit," once even going so far as to say that he would rather die than accept a statement that Jews were not Americans.

Marshall tried to fight anti-Semitism, as Rosenstock points out, on American, not purely Jewish grounds, not an easy task when other Jews, who also claimed to speak with authority, thought in purely Jewish terms, which, according to Marshall, antagonized the Christian majority and strengthened their anti-Jewish stereotypes.

Acting out of a mingled sense of *noblesse oblige*, and of self-preservation, this conservative corporation lawyer and staunch adherent of the Republican party threw himself into the fight for Jewish rights wherever he feared they were threatened, whether for the abrogation of the commercial treaty with Russia and the defense of Leo Frank, or against Henry Ford, the Ku Klux Klan, immigration restriction, the establishment of quotas for Jewish students in colleges, or even the release of Cecil DeMille's movie *King of Kings* in 1927. Nothing was too insignificant if Jewish rights, by which he meant human rights, were threatened.

Rosenstock offers us a saddening account of a man struggling vainly, albeit valiantly, against a rising tide of bigotry. Yet the lesson of Marshall's career is that only those who fight hard for their rights will ever get them; it is a sad commentary on the human race that this is so.

Kent State University

HAROLD SCHWARTZ

RAY STANNARD BAKER: THE MIND AND THOUGHT OF A PROGRESSIVE. By *Robert C. Bannister, Jr.* [Yale Publications in American Studies, Number 10.] (New Haven, Conn.: Yale University Press. 1966. Pp. xiv, 335. \$7.50.)

IN this first published full-length study of Ray Stannard Baker the author attempts to portray the man by concentrating upon his "mind and thought." Baker's mind, though far reaching, was, as Bannister acknowledges, not profound but rather limited and often superficial. His importance as a subject for historical study lies, as Baker himself realized, in his interaction with a rapidly changing

world. What Bannister's method produces is not a portrait but a highly selective scholarly sketch.

With his subtitle Bannister implies that Baker was representative of a progressive mentality, but by focusing so often on his subject in isolation he affords little proof of this assertion. While dutifully recording many of the books Baker read, the author tends to ignore the many personal and professional relationships that also contributed to the journalist's education and placed him in a progressive era.

Bannister's study is largely constructed around Baker's writings, especially the voluminous journals in which the reporter captured fleeting, often contradictory, thoughts and impressions. Such an unself-conscious source is invaluable, but it must be used with care. Bannister displays little of the caution that his criticism of Baker as a biographer should have suggested to him. For instance, the author is too ready to attribute many definitive changes in position to his subject.

Despite certain methodological limitations the volume does make a number of contributions. One of the author's aims is to fit David Grayson, the pseudonym under which Baker wrote nine volumes of homey, first person, country life sketches, into the larger life of his creator. Earlier interpreters either dismissed the Grayson work or uncritically accepted Baker's contention that he had completely segregated the expression. Though Bannister overuses the Baker-Grayson dichotomy as both a literary and interpretive device, he argues persuasively that the simplistic views of Grayson found their way into what the writer considered his more serious work. Employing a broad-gauge research technique, unfortunately confined to the early chapters, Bannister has treated well the education of the young Baker and has shown how the dominant ideas and sentiments at the turn of the century were mirrored in the work of the premuckraker.

After subjecting Baker's multivolumed biography of Wilson to criticism, Bannister concludes that the work caught the essence of its subject and that much of it has met the test of further scholarship. Perceptively, he evaluates the biography in part as a testament to the faith of Baker, who found in Wilson the ideas, attitudes, and goals that commanded his respect.

Some of the strokes of Bannister's sketch are sure and bold, but the volume remains only a sketch, for the limited and somewhat artificial approach of intellectual biography fails to encompass Baker. Need remains for a study that places the journalist in a more meaningful historical context.

University of North Carolina

JOHN E. SEMONCHE

OSCAR HAMMERSTEIN'S MANHATTAN OPERA COMPANY. By *John Frederick Cone*. (Norman: University of Oklahoma Press. 1966. Pp. xvi, 399. \$6.95.)

OPERA IN CHICAGO. By *Ronald L. Davis*. (New York: Appleton-Century. 1966. Pp. xi, 393. \$12.95.)

ALTHOUGH Cone and Davis approach their subjects differently, these two books have some notable characteristics in common. Apart from the quality of the texts, the appendixes listing the pertinent data of all performances of the Manhattan

Opera Company and of resident opera companies in Chicago have great reference utility. Each volume is based largely on newspaper sources, each is understandably fascinated with Mary Garden, and each considers the problem of operatic patronage in America.

This last concern—whether opera should be geared for the support of rich and socially prominent citizens or should seek to appeal to a broader base of music lovers—poses a central problem in *Oscar Hammerstein's Manhattan Opera Company*. The impresario, a German immigrant who got his start in this country as a cigar maker, apparently had two goals in life: to conquer the Metropolitan Opera Company with his own organization and to do so without the support of "society." The fight lasted four seasons, 1906–1910, and generated some of the finest productions this country has experienced. But Hammerstein's hope of a mass audience to support grand opera did not materialize, and, when he deliberately alienated the social leaders whose support was essential, the Manhattan was doomed. Cone's treatment of Hammerstein's outrageously inept handling of his society supporters reveals that for all his admiration for Hammerstein's undeniable achievements (especially in introducing modern French operas), he maintains judicious scholarly perspective. Similarly, he recounts Hammerstein's boorish and inexcusable methods of driving Lillian Nordica out of his company. In view of Hammerstein's immense entrepreneurial talent and artistic taste, some further analysis is needed of the personality characteristics that drove him to behavior so obviously inimical to the welfare of all he sought to build. Cone does not venture such analysis. What he presents is a well-written history, replete with scholarly apparatus. The University of Oklahoma Press is to be complimented on producing a handsome, reasonably priced volume.

The scope and claims of Davis' book are broader than Cone's, and his accomplishment less. He posits the opera house as a test tube for observing American intellectual and social trends and attempts to intertwine opera in Chicago with the history of the city and nation. The result is an unsatisfying patchwork of historical snippets and operatic information that often fails to demonstrate any significant trends or to establish the connection of Chicago opera with exterior events. For example, "The company closed its second season on January 21, 1917. Ten days later the German government announced its resumption of a policy of unrestricted submarine warfare." Considering just the operatic content of Davis' book, we get an interesting account of performances in Chicago from 1850 through the autumn of 1965. The information has been gleaned mostly from the daily press, and the chief weakness of the volume is that it rarely advances beyond the level of reporting on which it is based. Davis presents a chronicle, with little synthesis. There is no documentation, with only a brief note on sources. Contrived chapter titles and the anecdotal style clearly indicate the author's intent to present a popular, rather than a scholarly, account of Chicago opera. The foregoing criticism is not to deny the value of this work. *Opera in Chicago* makes a contribution in bringing together the basic data on the subject.

National Historical Publications Commission

WALTER RUNDELL, JR.

NEWTON D. BAKER AND THE AMERICAN WAR EFFORT, 1917-1919.

By *Daniel R. Beaver*. (Lincoln: University of Nebraska Press. 1966. Pp. xi, 273. \$6.50.)

THIS volume augments a burgeoning shelf of recent works on World War I, which may soon permit a general reinterpretation of America's role in that conflict. Beaver examines "the effects of the First World War on the American government, . . . the ways in which certain high policies were formulated in the Wilson administration, . . . the part [Secretary of War Newton D.] Baker played in the conflict, . . . and the effect of that experience on his mind and spirit." After noting Baker's policies in 1916 toward "preparedness" and the Mexican punitive expedition, the author discusses the first year of belligerency: selective service, Pershing's appointment to the AEF, and the adoption of basic policies. A chapter on industrial mobilization criticizes Baker's part in the process. Delays caused public complaints and congressional inquiry, but Baker preserved his place by reorganizing the War Department.

In 1918 Baker encountered field problems, particularly the European desire to "amalgamate" American soldiers into European armies to make quick use of them, a policy effectively resisted by Pershing and Baker. The Secretary's deference to Pershing encouraged the commander to arrogate all responsibilities to himself, including political matters. This tendency alienated the Chief of Staff, Peyton March, and engendered European criticism. When the Germans sought an armistice, Pershing attempted to fix American policy at the Supreme War Council, an initiative encouraged by Baker's previous flexibility and the commander's desire to prove the mettle of his troops, then engaged in the Meuse-Argonne offensive. (The author effectively refutes the impression that this operation was well handled.) Wilson himself rebuffed Pershing through Colonel House, supporting Beaver's view that he played an important part in military decisions. The sudden end of hostilities precluded more serious civil-military controversies.

Beaver concludes with an interesting discussion of the conflict between Baker's "progressive" background and his wartime responsibilities. War Department infringements on civil liberties and minority rights, particularly those of Negroes and pacifists, lost Baker his standing with liberals. He deemed restrictive policies necessary to victory. Beaver respects Baker, praising his capacity for growth, but stresses limitations stemming from his subject's espousal of "nineteenth-century economic liberalism," "states' rights traditions," and the special interests of the army.

Clearly written and well researched, the work easily surpasses earlier studies by Frederick Palmer and C. H. Cramer, although it retraces much familiar ground. It provides useful materials for a much-needed book not yet written: an over-all study of American military policy during World War I.

State University of New York, Stony Brook

DAVID F. TRASK

THE HILT OF THE SWORD: THE CAREER OF PEYTON C. MARCH.

By *Edward M. Coffman*. (Madison: University of Wisconsin Press. 1966. Pp. xi, 346. \$10.00.)

For the last eight months of the First World War General Peyton C. March was one of the great military leaders of the United States Army and of the nation. Professor Coffman focuses his account of March's professional career on the war-time period, when the general quickly brought order to the swollen and disjointed military headquarters of the army in Washington and made the General Staff such an efficient machine for getting men and supplies to France that victory on the battlefield came much sooner than almost anyone had anticipated. His strong leadership also gave eminence to the position of Chief of Staff, although the author properly concludes that March's fiat of August 1918 asserting the military supremacy of the position did not settle the point. President Wilson and Secretary of War Baker continued to treat General Pershing as supreme in France; it remained for Pershing, and General Marshall in World War II, to give the position of Army Chief of Staff pre-eminence.

The wartime success of March was a tribute to the excellence of his training and experience, and to his intelligence, decisiveness, and untiring energy. Working almost continuously during the war months for seven days a week until midnight, March drove others as he drove himself, and with a sharply military manner and a ruthlessness that, if they did not inspire affection, did get things done. But these qualities, together with the general's notable lack of tact in dealing with congressmen, helped to bring frustration and defeat to March's postwar military planning.

By June 1921, when Pershing succeeded as Chief of Staff and March retired from the army, the latter's wartime achievement was little remembered or appreciated. The best comment the *Army and Navy Journal* could make on March's retirement was that "history when written, in the proper perspective, will undoubtedly give him his due." At long last this work has fulfilled the prediction. In a concise, well-written, and thoroughly scholarly biography Coffman does full justice to March. The author's documentation and bibliography not only attest comprehensive research, but also provide a useful guide to the military sources and literature of World War I, the fiftieth anniversary of which is upon us. And his evidence fully validates the judgment of one of the general's bitterest congressional critics, when many years later he said: "General March was a great Chief of Staff during the war. He was a he-man."

Department of the Army

STETSON CONN

LETTERS ON THE LEAGUE OF NATIONS: FROM THE FILES OF
RAYMOND B. FOSDICK. By *Raymond B. Fosdick*. (Princeton, N. J.: Princeton University Press. 1966. Pp. xiii, 171. \$4.50.)

THIS first of a projected number of supplementary volumes of *The Papers of Woodrow Wilson* is an auspicious, though small, beginning. It contains writings by Fosdick, who from June 1919 to January 1920 served as Undersecretary General of the League of Nations during its provisional period of organization. He was

the only American to hold such a high position, and it brought him into contact with those figures preoccupied with building the League. It also thrust him into the Senate struggle over the Treaty of Versailles.

Scholars can find information on a variety of subjects. The letters reveal the problems of creating an operating League and disclose from an insider's view the duties and desires of a new class of public employee: the international civil servant. There are references to educational and labor agencies, territorial problems, mandates, economic issues, and disarmament. The dreams of the internationalists also appear, as well as their differences, for one finds revealing dialogues with representatives of liberal and conservative viewpoints. The most startling passages disclose the cautious and uncooperative attitude of the State Department toward the League under Lansing in 1919.

Most of the volume deals with the treaty fight. It adds little that the specialist does not know, but reveals Fosdick as an acute and informed observer. He happened to be in Washington in charge of an international labor conference, and references to this unhappy episode also reflect current conditions and attitudes. The last twenty pages cover Fosdick's advocacy of the League between 1920 and 1924. They complete the picture of a dedicated internationalist filled with zeal for his cause and passionately loyal to Wilson.

Although the book is generally well edited, the brief identifying footnotes are often inadequate, occasionally failing to supply even the first names of persons. One might also have wished for a fuller sketch on Fosdick in this otherwise informative work.

University of Akron

WARREN F. KUEHL

PREACHERS PEDAGOGUES & POLITICIANS: THE EVOLUTION CONTROVERSY IN NORTH CAROLINA, 1920-1927. By *Willard B. Gatewood, Jr.* (Chapel Hill: University of North Carolina Press. 1966. Pp. viii, 268. \$5.95.)

UNLIKE many other recent studies of rural, evangelical movements in the 1920's, Willard Gatewood's book is not marred by condescension, is not an exercise in demonology, and does not attempt to score belated points against the opponents of modern currents in science and theology. Carefully researched and judiciously presented, Gatewood's study is the most intensive and successful account of the evolution controversy in the 1920's yet published.

The most salient defect of the book is the author's failure to discuss in sufficient detail why militant fundamentalism rose at this particular juncture in North Carolina. The general explanations he gives are interesting and probably valid, but state studies present an opportunity for specificity that Gatewood does not take complete advantage of in this area. Nor does he present sufficient background material to allow the reader to appreciate fully the social, economic, and political context in which the evolution controversy was waged. Thus, when he shows that legislators from mountain counties gave the unsuccessful antievolution bill its heaviest vote, followed by those from the Piedmont and the coastal plains, the

meaning of his analysis is lost on anyone not already well versed in North Carolina history.

These shortcomings are compensated for by Gatewood's serious and detailed treatment of antievolution thought. With a fine talent for selecting cogent and illuminating quotations, he skillfully demonstrates its relationship to traditional rural Protestant values and to southern racial beliefs and mores. He places the movement in the context of the period's predilection for legislation dealing with alcohol, sex, and Communism and shows how the theory of biological evolution came to epitomize the totality of error.

As his title implies, Gatewood is equally concerned with the thought and action of those who were the primary targets of the antievolutionists. The lengthy sections of his book that deal with the efforts of Presidents William Louis Poteat of Baptist-affiliated Wake Forest College and Harry W. Chase of the state university to preserve the academic integrity of their institutions against the onslaught of denominational and legislative attacks are admirably done and constitute an important chapter in the history of academic freedom that well might be made required reading for every college administrator in the country.

University of California, Berkeley

LAWRENCE W. LEVINE

BYGONES I CANNOT HELP RECALLING: THE MEMOIRS OF A MOBILE SCHOLAR. By *J. Fred Rippy*. (Austin, Texas: Steck-Vaughn Company. 1966. Pp. ix, 195. \$5.95.)

THIS modest memoir by one of the pioneer historians of Latin America and United States foreign policy justifies the description on the jacket, "the rollicking recollections of a genial scholar." It should delight the countless former students of J. Fred Rippy, who began to teach in a country school in Tennessee while still a child and continued even after his retirement in 1958 from the University of Chicago.

Rippy's evocation of rural Tennessee in his first ten years and, after 1902, of north Texas farm life is a charming social document. His account of his years at Southwestern, Vanderbilt, and Berkeley from 1909 to 1920 continues the autobiography in a relaxed conversational manner, but offers a sobering picture of university education of the times.

He came into his own as a popular professor and productive scholar at the University of Chicago between 1920 and 1926. Yet here and elsewhere in the book Rippy stresses the discomfiture southerners often felt in cosmopolitan industrial centers. He was also distressed to discover the existence of faculty politics, a topic he treats bluntly but without rancor. In 1926 he went to Duke University in order to serve and live in "my Southland." His remarkable scholarly output brought him to the top of his profession. He found Duke somewhat stifling and in 1936 returned to Chicago in the hope of contributing more to his beloved South by training students from that region who would go back as educators. This hope did not materialize, for southern students failed to appear in large numbers. Rippy felt that the university lost prestige and appeal under Chancellor Robert M. Hutchins, and he lamented the change in the character of the student body and

the tendency of his colleagues to seek influence and position in government. Yet he labored on, lecturing ever-larger classes, guiding innumerable thesis writers, and publishing thirteen books of his own and fourteen in collaboration, along with a torrent of articles.

Pungent as his comments on individuals, institutions, and trends are, Rippy is never unkind. His amiability and love of people shine in these pages, as they have throughout his long career. Since he has contributed so much to the history of the Americas and has always been thoughtful and often original and profound, it is hoped that he will follow this delightful memoir with an intellectual autobiography.

New York University

JOHN EDWIN FAGG

KOHLER ON STRIKE: THIRTY YEARS OF CONFLICT. By *Walter H. Uphoff*. (Boston: Beacon Press. 1966. Pp. xx, 449. \$7.50.)

AN exceedingly detailed case study of the longest and perhaps bitterest labor-management dispute in American history, this book painstakingly documents the unwillingness of a large corporation, Kohler, to accept the twentieth century. It tells of the taming of a nineteenth-century frame of mind by tenacious local workers aided by the powerful United Automobile Workers (AFL-CIO) and finally vindicated by the full authority of the NLRB and federal courts.

The particulars are not typical of recent labor history. Twice Kohler resisted unionization, and extended strikes resulted: 1934-1941 and 1954-1960 (with final settlement of outstanding issues only in December 1965). Most remembered for the July 1934 death of two strikers and the wounding of forty-seven others, who were mostly shot in the back, the first strike resulted in an "independent" union, the Kohler Workers Association. In 1954, significantly sparked by formerly loyal KWA leaders, Kohler workers voted overwhelmingly for the UAW. But company hostility caused a second strike that attracted nationwide attention in the placid Eisenhower era. The company made "more elaborate, far-reaching 'defenses' against a possible strike than are recorded anywhere in modern American industrial history." The union responded in kind. Company use of private detectives, purchase of arms and ammunition, and manipulation of the local power structure together with mass picketing, boycotts, and petty acts of disorder and even serious violence suggested the bygone era of primitive industrial warfare. The conflict made Kohler the darling of the far Right, cost the UAW \$10,188,961.77 between 1954 and 1957, and received notice from Senator McClellan's committee. NLRB hearings between 1955 and 1959 added particularly incriminating evidence about unfair labor practices of the company, but it resisted all efforts by outside parties to effect compromise. Only final federal court action forced it to accept the twentieth century and the fact that industrial unionism and collective bargaining were not a plot to destroy "Republican Virtue" but a rather normal, if often painful, adjustment to modern civilization.

Uphoff overwhelms the reader with massive evidence and much petty detail. He is not a historian but a labor economist, and his study shows it. His interest

in Kohler has been long and sustained. Though he sympathizes with the workers he states the company case fairly despite its unwillingness to share its records with him. The narrative and even the analysis suffer from the case study focus. The organization is too schematic, and there are, too frequently, long, ill-digested extracts from newspapers, personal interviews, and NLRB and court hearings. Thus, the evidence is often fascinating, but the book is often dull. Additional study of this dispute is not needed.

Center for Advanced Study in the Behavioral Sciences

HERBERT G. GUTMAN

THE WAR DIARY OF BRECKINRIDGE LONG: SELECTIONS FROM THE YEARS 1939-1944. Selected and edited by *Fred L. Israel*. (Lincoln: University of Nebraska Press. 1966. Pp. xxv, 410. \$7.50.)

PROFESSOR Israel has made a really significant contribution to the study of recent history in this well-edited selection from the diary of a second-level wartime administrator. And the University of Nebraska Press deserves special credit for having undertaken a kind of publishing risk that one hopes more university presses will be able and anxious to invite in the future.

The significant things about the *War Diary of Breckinridge Long* are its fullness, frankness, and the fresh self-confidence with which its author asserts his views and news. Although he was never a great power in the New Deal camp, Long held a special vantage point from which to view the excitement of wartime Washington. Somewhat independent of both the diplomatic and the "liberal" establishments, Long was associated by friendship, sentiment, and politics with both the southern and the wealthy conservatives of the Democratic party. Yet he was not their captive, any more than he was the puppet of the genial President who had been his great good friend when they had been young together in the back reaches of the Wilson administration. That the *Diary* is a reliable, firsthand document is clear in the wealth of detail, the slapdash style, the original spelling, the special mixture of personal and public items, the frank expression of views less than flattering to the author. But the document is, of course, the testimony of a participant. It tells much of Long's own prejudices, principles, and passions. He emerges here as a more hesitant figure, a more limited intellect than one would have expected, concerned much with horses, Princeton, and the genteel life of a Maryland farmer.

But his *Diary* provides much fresh information on a variety of significant topics: on the power structure of the State Department; on the tensions between the official diplomatic establishment and Roosevelt's personal one; on the fateful confusions and callow rivalries of pre-Pearl Harbor Washington; on Hull's long, private conferences with Admiral Nomura; on the President's health and fatigue; on Long's own role in great matters. Long's uninhibited frankness and the editor's sensible, sensitive editing combine to make this volume one which the interested reader may enjoy and yet one on which the specialist may properly rely.

State University of New York, Binghamton

ALFRED B. ROLLINS, JR.

SSHA V VOENNYE I POSLEVOENNYE GODY (1940-1960) [The USA during the War and the Postwar Period (1940-1960)]. By V. I. Lan. (Moscow: Izdatel'stvo "Nauka." 1964. Pp. 685.)

ANGLO-AMERIKANSKIE OTNOSHENIIA V GODY VTORY MIROVOI VOINY, 1939-1941 GG. [Anglo-American Relations during the Second World War, 1939-1941]. By L. V. Pozdeeva. (Moscow: Izdatel'stvo "Nauka." 1964. Pp. 452.)

AMERIKANSKAIA ISTORIOGRAFIIA GRAZHDANSKOI VOINY V SSHA (1861-1865) [American Historiography of the Civil War in the USA (1861-1865)]. By I. P. Dement'ev. (Moscow: Izdatel'stvo Moskovskogo Universiteta. 1963. Pp. 349.)

OF the three works considered here, two need little serious attention. The volume by V. I. Lan on the history of the United States from 1940 to 1960 is, despite its length, a frail structure of a usual Soviet type, arguing that the United States during that period represented little more than a country of poverty, violence, and corruption, ruled over by marauding cliques of "capitalists," who used the machinery of a subservient and spineless government in their attempts to extend American rule throughout the world. In advancing this argument Lan draws on a heterogeneous mass of materials which, according to his footnotes, he found in such American sources as *Life*, *U. S. News and World Report*, and the *Saturday Evening Post*, or in books written as campaign biographies of presidential candidates. Many pages, containing startling and serious statements about American political figures and governmental policies, have either no source citations at all or but one somewhat extraneous reference to a peripheral point. It must therefore be said that, despite the sponsorship of the Institute of World Economics and International Relations and the august Academy of Sciences of the USSR, this is little more than an extended, ill-tempered political pamphlet.

L. V. Pozdeeva's work on Anglo-American relations in the years 1939-1941 is seemingly a more scholarly work, for it draws on a wide variety of official documents and memoir literature in its examination of the way in which England and America came to cooperate with one another in opposing Germany. Since the longest single chapter deals with Anglo-American attitudes toward the Soviet Union, however, it is strange to find little reflection of Soviet documents on the subject, not to mention the total obliteration of such personalities as V. M. Molotov or the overshadowing of V. I. Stalin, as far as the number of citations in the index may serve in this relation, by W. Z. Foster. Despite these peculiarities, this volume, sponsored by the Institute of History of the Academy of Sciences, may offer some small guidance to those who seek to determine the Soviet view of the development of cooperation between Great Britain and the United States before Pearl Harbor.

The third volume, I. P. Dement'ev's study of American historiography of the US Civil War, published by Moscow University, is considerably more interesting for it is one of the first Soviet studies of the development of American historical thought, as well as an example of the breadth of Soviet concern with our Civil War, extending to more than eighty books, articles, and reviews in the Russian language. Dement'ev examines the varying historical assessments of the Civil War

and its causes, which have been made by historians, or would-be historians, ranging from Frederick Douglass, or John A. Logan, to James F. Rhodes, Allan Nevins, Avery Craven, and Foster. The author, who evinced commendable zeal in carrying out his research, is, as one might expect, quick to praise the "progressive" historians and blame the "reactionary" ones, although his section on American Marxist historians does not indicate that he is entirely satisfied with their writings. There are a number of absurdities, such as the attempt to make the so-called "Southern Bourbon" school of historians directly dependent on "monopoly capital," but Dement'ev's volume must be considered one of the more significant Russian works on a period that, as other Soviet writings suggest, is seen as central to any analysis of the American experience.

Library of Congress

ROBERT V. ALLEN

THE CHEMICAL WARFARE SERVICE: CHEMICALS IN COMBAT. By *Brooks E. Kleber and Dale Birdsell*. [U. S. Army in World War II: The Technical Services.] (Washington, D. C.: Office of the Chief of Military History, U. S. Army. 1966. Pp. xvii, 697. \$5.25.)

LIKE many of the volumes of the "U. S. Army in World War II" series, this is as much a document as it is a secondary history. Its extreme range passes from minute concrete detail about organization and combat activity to informed speculation in the last five pages as to why the belligerents did not use poison gas during World War II. Most readers will skim much of the detail, but some will find it to be resource material. No perceptive person will come away from the book without some benefit.

The Chemical Warfare Service, which became an integral part of the military establishment in 1920, was unique. It had to prepare for a gas war which, as matters developed, was never fought, and at the same time supply, train, and command several types of combat troops. It was the only technical service having troops armed with special weapons provided by itself. Half the book is devoted to the history of the CWS combat troops. They were of three types: smoke screen units, 4.2-inch mortar units, and flame throwers, both portable and mechanical. The War Department did not authorize the use of high explosive ammunition in the 4.2-inch "chemical" mortar until March 19, 1943, but after that time this mortar became used for close support. At short ranges it could place four and one-half times as much high explosive upon a target as a 105-millimeter howitzer, but it weighed 305 pounds as opposed to 4,000 pounds for the howitzer. It was the star combat performer of the CWS. Besides the detailed narrative of their operations, there are critical evaluations of the effectiveness of the three combat elements.

The Chemical Warfare Service was responsible for the development and production of incendiary bombs used by the US Army Air Force. These bombs were one of the most brutal and most effective weapons of the war. Because the story of their use belongs to the air force, the authors devote only twenty pages to them.

This volume, like the others in the series, is skillfully planned and assembled.

In seven hundred pages I did not find one typographical error. Abbreviations are explained, the bibliographical essay is exhaustive, and the index is useful.

University of Florida

JOHN K. MAHON

THE MEDICAL DEPARTMENT: MEDICAL SERVICE IN THE MEDITERRANEAN AND MINOR THEATERS. By *Charles M. Wiltse*. [U. S. Army in World War II: The Technical Services.] (Washington, D. C.: Office of the Chief of Military History, Department of the Army. 1965. Pp. xxvi, 664. \$5.00.)

"The soldier struck down on the field of battle, if his wounds are more than superficial, may count his life expectancy in hours, or even minutes, unless he receives prompt medical aid." So begins Charles Wiltse's account of the performance of the Medical Department of the United States Army in the Mediterranean theater during World War II. It is a story of careful planning, smooth organization, battlefield ingenuity, and brilliant improvisation that concludes with the remarkable statistic that of 112,000 personnel wounded in that theater, only 4,000 died.

The Mediterranean proved to be an important proving ground for the Medical Department, and the experience gained there was invaluable on other fronts. Medical concepts that had worked reasonably well in the very different experiences of World War I were altered or abandoned in the light of the Mediterranean experience. Important new techniques, such as the treatment of most psychiatric casualties in the combat zone and the use of whole blood and penicillin in forward surgery, were tried here. Jeeps, half-ton trucks, airplanes, and even mules were pressed into service in transporting the wounded from inaccessible places to ambulances. Preventive medicine won important triumphs by ending the typhus threat in Naples in 1943, reducing malaria rates, and keeping such local scourges as cholera and bubonic plague under control.

This volume is one of the series on the "U. S. Army in World War II" and the second to appear in the Medical Department subseries on "The Technical Services." It is a sequel to the volume published in 1956 on hospitalization and evacuation in the Zone of Interior and will be followed by two more dealing with medical services in the European theater of operations and the Pacific-Asiatic areas. It differs from the clinical series published by the Office of the Surgeon General in its concentration on the support given by the Medical Department to actual combat operations.

The author has done a first-rate job of collecting and analyzing the wide-ranging sources available for his study. He has done a painstakingly careful, detailed, comprehensive piece of work. The value of the study is enhanced by good maps, helpful tables, and a good selection of photographs. If the organization seems unimaginative and the style a bit lifeless, perhaps part of the responsibility may be laid to the problems encountered in writing official, bureaucratic history. Wiltse writes for the specialist and the professional, and for these this volume will prove a worth-while addition to the literature of military medicine.

University of Cincinnati

THOMAS N. BONNER

POSTWAR DEFENSE POLICY AND THE U. S. NAVY, 1943-1946. By Vincent Davis. (Chapel Hill: University of North Carolina Press. 1966. Pp. xv, 371. \$7.50.)

ANYONE who has examined the navy's efforts during the 1930's to prepare its own component of the Orange Plan (envisaging a war with Japan) has seen how difficult it was then for seasoned admirals to abandon traditional concepts of sea power. Yet, a year after the stirring victory of Midway, a naval air battle that erased the shame of Pearl Harbor, naval leaders, awakened to the broader implications of sea power, began to divide their professional concern between the prosecution of the war and the growing demands of postwar naval planning. It is to the second of these matters that Dr. Davis has directed his attention.

In 1943 the navy officially assigned its postwar problems to a special planning section, headed by retired Admiral Harry E. Yarnell, who had reached the climax of his public fame as Asiatic Fleet Commander during the *Panay* crisis. The Mahan-minded admiral developed defense concepts for the postwar navy that later events were to render obsolete, at least in part, and his initial plan was soon overshadowed by others that were developed in response to changing circumstances.

Davis has placed his main research emphasis on the three years following the Yarnell draft, when naval planners had to consider a number of factors that were to influence the navy's postwar defense policy. These included public clamor for quick demobilization; the emergence of the USSR as a potential enemy; the demands of naval aviators for a prominent role in the postwar navy; the proposed unification of the armed services; and, finally, the impact of atomic power upon future defense considerations.

This study is well written, thorough, and lucid, offering an orderly account not only of what happened in naval postwar defense planning but how it happened. It also demonstrates that the navy was able to accommodate itself to the pressures of change. Davis' extensive bibliography includes primary sources drawn from unpublished naval records and documents, personal papers, and interviews with over a hundred individuals. Although perhaps too specialized for the general reader, the work is essential for the naval historian. It presents an instructive analysis of that significant but often neglected phase of naval history that evolves behind the thunder of the guns, and it deserves a special place in the historical literature of World War II.

Saint Peter's College

THADDEUS V. TULEJA

FOREIGN RELATIONS OF THE UNITED STATES: DIPLOMATIC PAPERS, 1944. Volume IV, EUROPE. [Department of State Publication 8067.] (Washington, D. C.: Government Printing Office. 1966. Pp. vi, 1473. \$4.75.)

THIS tome contains a rich mine of source material. As the military vise closed around the Axis, authoritarian countries such as Spain and Portugal were forced into an ever more benevolent neutrality toward the United Nations. The Rumanian surrender signed at Moscow, the tedious details of which are presented

here, foreshadowed paramount Soviet interest in the eastern Axis satellites. Lengthy accounts illustrate the unsuccessful attempts of Sweden and Switzerland to retain a modicum of trade relations with the *Reich*. The section on Yugoslavia highlights the Allied decision to support Tito and his partisans and illuminates Tito's diplomatic skill in underplaying his Marxist orientation.

The most important documents here released delineate American-Russian relations. This portion contains impressive analyses of Soviet conduct by such informed observers as Harriman and Kennan in Moscow and Bohlen and Durbrow in Washington. A healthy skepticism of Soviet good faith existed on both sides of the water. American diplomats understood Russian xenophobia and the Kremlin's appetite for territorial expansionism, and they realized that our generosity in lend-lease support was leading to Soviet stockpiling. Yet pessimistic reports about Russian behavior were invariably interspersed with guarded optimism. Nothing here published challenges the consensus of historians that, despite some blunt warnings from his advisers, Roosevelt wished to keep the American record so spotless that Stalin would overcome his ingrained suspicions of Western motives.

It is unlikely that these documents will sharply reverse cardinal historical judgments. They supply, however, a myriad of valuable details and insights. As the war drew to a close, Washington exerted relentless pressure upon remaining neutrals, not fearing to use the black list or threats of postwar retaliation. Once the possibility of Nazi retaliation vanished, we expected every feasible concession from the neutrals. When these harassed nations served notice that they wished to keep their record of neutrality unsullied for the future, Hull replied that henceforth a revamped system of collective security would prevent wars of aggression. In any event, he added, the conditions of modern warfare had outmoded the very concept of neutrality. If the Secretary was mistaken in his initial surmise, events seem to have justified his afterthought.

State University of New York, Buffalo

SELIG ADLER

THE TRUMAN PRESIDENCY: THE HISTORY OF A TRIUMPHANT SUCCESSION. By *Cabell Phillips*. (New York: Macmillan Company. 1966. Pp. xiii, 463. \$7.95.)

CABELL Phillips, veteran Washington correspondent of the *New York Times*, has replaced Jonathan Daniels and Alfred Steinberg as the author of the best journalistic account of Truman's presidency. This is not a biography. In fact, the biographical chapter on Truman's life before 1945 is filled with errors. Daniels' *The Man of Independence* (1950) remains the best account of that subject, but that book comes to an end with Truman's victory in 1948 while Phillips carries the story of the President, his staff, and his times to 1953. Steinberg, in *The Man from Missouri* (1962), also deals with both of Truman's terms in the White House, but depends more heavily upon earlier Truman books. Phillips also makes better use of the available scholarly literature, especially the works of Herbert Feis. Phillips neglects important subjects and does not present a novel interpretation, but he does contribute significant new information.

The book reflects the high estimate of the Truman presidency that developed

during the Eisenhower years. Truman, the author asserts, was a "great President." In support of this interpretation, he emphasizes Truman's record in foreign affairs and his impact upon the presidency. The account ignores the "revisionism" now emerging that suggests a more effective President could have accomplished more at home and raises doubts about the need for and value of the containment policy. (For an introduction to this interpretation, see *The Truman Administration: A Documentary History*, recently edited by Bernstein and Matusow.)

The author's contributions depend chiefly upon help he received from important members of the Truman administration. Clark Clifford, Dean Acheson, and others discussed the subject with him and also supplied materials from their files. As a consequence, he is able to reveal, for example, the significant role played by a group of liberal advisers in shaping Truman's re-election strategy. This subject and the entire story of Truman's White House staff deserve more thorough and systematic treatment.

Phillips made little use of the Truman Library. Alienated by restrictions upon access to materials there, he now denounces presidential libraries as "sentimental mausoleums" and suggests that presidential papers should be transferred to Washington. His experience justifies neither the attack nor the suggestion. Truman's policy on access to his papers does create difficulties, but it would govern their use in any location. Furthermore, the attack ignores the publications that draw upon another presidential library, the Roosevelt Library, and the research being done by scholars willing to work through the valuable papers available in the Truman Library.

University of Missouri

RICHARD S. KIRKENDALL

GEORGE C. MARSHALL. By *Robert H. Ferrell*. [The American Secretaries of State and Their Diplomacy, Volume XV.] (New York: Cooper Square Publishers. 1966. Pp. xiv, 326. \$7.50.)

THIS new volume in the series "The American Secretaries of State and Their Diplomacy" is devoted to the only professional soldier who ever served as Secretary of State. General George C. Marshall's two short years as head of the State Department spanned a revolution in American foreign policy. The Truman Doctrine, the Marshall Plan, the Berlin blockade, the Baruch plan, the Rio treaty, the fall of Nationalist China—all involved an intensity of American intervention in world affairs that had no peacetime precedent in the past and from which there would be no turning back in the future.

Professor Ferrell's book on the Marshall secretaryship is well organized and lucidly and readably written. He covers the main issues sensibly if without great originality, and his judgment of men and events is candid and fair minded, though by no means always persuasive. I am not clear why he should be so obscure about James F. Byrnes's resignation as Secretary of State since both Byrnes and President Truman have written volubly, if in sharp disagreement, about the episode. In composing the book, Ferrell drew on manuscripts as well as published sources, but the manuscript citations, though they lend verisimilitude to the narrative, do not alter its familiar outlines.

The mystery remains as to what sort of Secretary Marshall was, and this book does little to solve it. By Ferrell's account, Marshall was a relatively passive executive, relying, in the military manner, on his staff to identify issues and recommend courses of action. While this was doubtless true, Marshall's military experience was also responsible for significant administrative innovations in the State Department—especially the invention of the secretariat as a means of internal coordination. But this was a matter of mechanics, rather than policy. "He believed in staff work," Ferrell writes. "... He did not resort to his own private experts, least of all his personal expertise." At the Moscow Conference in 1947, "he left preparation of policy almost entirely to his staff." He had virtually nothing to do with the Truman Doctrine and was no more than the spokesman for the great plan that bears his name. Except for China and the Rio and Bogotá Conferences, he displayed no concern for the underdeveloped world. Even in the military field he showed no great interest in atomic weapons, their control, or their impact on strategy, and, though he later complained that as Secretary of State he had only "1½ divisions over the entire United States" to back up his policy, he did nothing at the time, as Ferrell points out, to argue against the decline in conventional forces, to expose the illusion of victory through air power, or to resist the disastrous defense budget of 1949.

But Marshall became Secretary of State at an exceptionally lucky time. His President was a man of courage and decision; his department, for a man so dependent on his staff, had been revitalized by a transfusion of fresh blood during the war and was enjoying a brief moment of creativity before succumbing to McCarthyism, elephantiasis, and John Foster Dulles in the 1950's. In Dean Acheson and Robert Lovett Marshall he had lieutenants of brilliance and purpose. Will Clayton was his Economic Undersecretary; George Kennan, chief of his Policy Planning Staff. Reliance on such a staff could hardly fail, and Marshall's triumph lay in the austerity of commitment he evoked from those around him. It was a triumph not so much of vision or of ideas as of character. As Kennan wrote after Marshall's death, he was ever "the image of the American gentleman at his best—honorable, courteous, devoid of arrogance, exacting of others but even more of himself, intolerant only of cowardice, deviousness and cynicism."

City University of New York

ARTHUR SCHLESINGER, JR.

HOUSING REFORM DURING THE TRUMAN ADMINISTRATION. By

Richard O. Davies. (Columbia: University of Missouri Press. 1966. Pp. xiv, 197. \$5.50.)

THIS case study of one representative segment of President Truman's Fair Deal program is a model monograph: it keeps its focus on the issues at hand without neglecting the full historical context; it renders complex legislative and administrative maneuvers intelligible; it holds the reader's attention; its judgments are balanced.

Professor Davies contends that Truman quickly assumed command of a comprehensive housing reform program whose origins lay deep in progressivism but whose major legislative accomplishment had been the Housing Act of 1937. The

pent-up demand for housing in the postwar years provided a broad popular base of support for the Truman program, while congressional backing could be mustered from leaders in both parties, particularly from those representatives who came from urban districts. Opposition came from organized realtors who claimed to see the threat of socialism in all government housing proposals. The President and Raymond M. Foley of the National Housing Agency were not unsympathetic with the needs and the views of the housing industry, but differed with them in insisting on a broad program of public housing and slum clearance. Operating in a period of political stalemate, Truman made the most of his opportunities during the campaign of 1948, successfully tagging the Eightieth Congress with having frustrated the national will, and was able to score in the Housing Act of 1949 one of his very few legislative victories. But by 1949 the struggle for housing reform had spent itself, the President was soon distracted by the events of the Korean conflict, and the one man who might have carried public housing through to full implementation, Senator Wagner, was forced to withdraw from the Senate because of ill-health. Consequently, only a fraction of the public housing units authorized in 1949 were ever constructed. The author concludes, moreover, that the "major source of disillusionment" with public housing was its failure "to fulfill the high aspirations of housing reformers—to reduce the social disorders of crime, vice, broken families, and juvenile delinquency." Public housing too often merely institutionalized the slums; poor housing, it became clear, was as much effect as cause of poverty and social disorganization. Truman's chief contribution, therefore, may have been the establishment of decent housing as a reasonable and essential goal toward which American democracy might strive.

The notes provide a useful guide to the evidence, but the bibliography would have been substantially more helpful had it included annotations.

University of Minnesota

CLARKE A. CHAMBERS

USA IN FOCUS: RECENT RE-INTERPRETATIONS. *Sigmund Skard*, Editor. [Scandinavian University Books. Publications of the Nordic Association for American Studies, Number 2.] ([Oslo:] Universitetsforlaget. 1966. Pp. 200.)

Two years after the second triennial conference of the Nordic Association for American Studies, the eight main papers, along with the remarks of some commentators, are available. Cast under the general theme of "American Civilization: Recent Re-interpretations," the major essays range widely, from Henry Wasser's survey, "The Changing Intellectual Climate in the USA since World War II," and Nathan Glazer's reflections, "Recent Discussion of the Social Structure of the United States," to Charles R. Anderson's perceptive "Wit and Metaphor in Thoreau's *Walden*," and Roger Asselineau's skimpy outline, "Ishmael—or the Theme of Solitude in American Literature." Among the others are two uneven articles by Sigmund Skard and Robert Spiller on the nature and problems of American studies.

Despite occasional insights, most of the major papers, which were designed for a European audience, are likely to be disappointing to American scholars. Gen-

erally the essays are restatements of familiar interpretations or expressions of the "conventional wisdom." An example of the former is C. Vann Woodward's "The Obsolescence of the New South," a summary of ideas he has brilliantly developed elsewhere. Unable to escape the economic disadvantages of the ante bellum years, the South, writes Woodward, remained short of capital, attracted the least desirable industries, and continued in bondage to the North. Ironically, it was this New South that created the "plantation legend" and recast race relations. Despite rapid economic growth in recent years, concludes Woodward, the "ghosts" of colonial economy and traditional society still haunt the South, which resists the Negro's struggle for equality.

In "Tensions in American Foreign Policy" in the twentieth century, John M. Blum gracefully articulates the "conventional wisdom." Though noting the conflicts between moralism and realism, between aspiration and possibility, and sometimes between rhetoric and reality, he praises the United States for recent generosity, for supporting UN forces, and for maintaining the "custodianship of deterrence." He blames Russia exclusively for the early cold war and neglects the fact that the crushing of the "spirit of Yalta and Potsdam" was a two-way affair. He also quickly by-passes Vietnam, denies America's early hostility to the Cuban revolution, and concludes that Castro moved into the Soviet orbit in spite of American offers of aid and friendship. On balance, then, this interpretation uncritically reaffirms much recent foreign policy and finds it imbued generally with the highest national values.

Stanford University

BARTON J. BERNSTEIN

WAR FOR THE COLORADO RIVER. Volume I, THE CALIFORNIA-ARIZONA CONTROVERSY; Volume II, ABOVE LEE'S FERRY. By *John Upton Terrell*. [Western Lands and Waters Series, Numbers 4 and 5.] (Glendale, Calif.: Arthur H. Clark Company. 1965. Pp. 325; 323. \$17.50 the set.)

You may as well forget about objectivity. The treatment is full—two three-hundred-page volumes covering less than a decade—and accurate. But throughout both volumes the author has prepared a brief for California against alleged sinister machinations by Arizona, the Bureau of Reclamation, and the so-called upper basin states. In other words, this is a modern-day economic Western, in which the "goods" and the "bads" wear as unmistakable identification badges as white and black hats.

Although the controversy is almost as old as the ownership and early exploration of most of the Colorado River by the United States, the study here begins with the Eightieth Congress and Arizona's efforts to get bills passed that would divert a larger share of the lower Colorado's water across its arid lands. California, with a heavily Republican delegation, opposed any such legislation, holding out for a Supreme Court decision on rights and claiming that Arizona was afraid of a legal inquiry. Other western states were and are heavily inclined toward Arizona. Even Arkansas, which, despite hanging judges and rivers that rise in the Rockies, hardly classifies as "West," insisted on backing Arizona and trying to join whatever interstate compact emerged. California held that the na-

tion's tax money should be invested in water for the millions needing it now, not in land whose people were yet unborn or unmoved.

The author's stance is forthright. He accuses the upper basin states of "unjustified attacks" on California; the Department of the Interior of sponsoring a "grand example of the great program which the Reclamation Bureau had formulated for wrecking the reclamation laws and setting itself up as the supreme power in the West"; and Senator McFarland of Arizona of "sheer hypocrisy" in a "deceptive scheme." However, he is fighting an ex post facto action, for the combination of western congressmen, President Eisenhower, and a Californian named Richard Nixon who did not stand hitched to the California position guaranteed success for the Colorado River Storage Project in 1956. As Terrell observes, "Nothing but a directive from the Almighty could have stopped such a powerful coalition of political forces from achieving victory."

This book is a highly interesting narrative, and the author has undoubtedly searched through and presented most of the available evidence. But one should read it as a tract, recognizing all the while that some tracts are not without their historical value.

University of Texas

JOE B. FRANTZ

KENNEDY. By *Theodore C. Sorensen*. (New York: Harper and Row. 1965. Pp. viii, 783. \$10.00.)

THE Kennedy administration seems destined to be distinguished, among its several notable characteristics, for the quality of the memoirs it has inspired. Even if there were no other accounts forthcoming beyond Theodore Sorensen's *Kennedy* and Arthur M. Schlesinger's *Thousand Days*, historians would have reason to consider themselves fortunate. These two memoirs will always be prime sources on the Kennedy years. Upon its appearance, Sorensen's *Kennedy* received unusual acclaim, only to seem eclipsed a few weeks later by Schlesinger's vivid, eloquent, wide-ranging, and candid masterpiece. But the merited success of Schlesinger's *Thousand Days* has by no means diminished for historians the value of Sorensen's study. A memoir is so innately personal, almost inevitably presenting and defending one man's point of view, that a historian must avail himself of as many first-rate memoirs as possible and may be grateful for even mediocre ones.

Sorensen's *Kennedy* is assuredly first rate, and while it overlaps other accounts, contains much that varies both in facts and viewpoint, and considerable that is as yet unique in print. Its special merit is the viewpoint—that of one of Kennedy's earliest and closest assistants who was a firm liberal. No one person was or could be privy to all the multitude of affairs involving the President; no single memoir or even group of memoirs can entirely duplicate in content, let alone in interpretation, the one Kennedy himself would have written had he lived. And almost inevitably a presidential memoirist tends to emphasize not only his own role but the significance of that segment of White House affairs in which he was involved. Because of his closeness, Sorensen writes from firsthand knowledge about most of the major concerns of the Kennedy administration. His style and viewpoint illuminate his contribution as special counsel to Kennedy. He is always loyal, sel-

dom even mildly critical, and presents what he has to say in the carefully organized, rather unadorned style of a competent government report. These pages seem the work of Kennedy's administrative lieutenant rather than his speechwriter. The low-key, matter-of-fact exposition creates confidence in the author's accuracy. The impersonal way of writing, such as the relation of incidents without the naming of names, also smacks of the government report and is irritating because it gives the impression that Sorensen is placing discretion ahead of candor. Historians can amuse themselves by taking a fragment from the prologue, Kennedy's remark during the Cuban missile crisis, "Did you notice what Doug Dillon said about the Jupiters?" and fitting it to the main account of the crisis where Dillon's name is missing, but the views of an unnamed person concerning Jupiter missiles are recorded. Inevitably, too, historians will compare Sorensen's accounts with differing or fuller details in other memoirs. Although newspapermen might make much over Schlesinger's specifics about Secretary of State Dean Rusk compared with Sorensen's generalities, Sorensen's discretions have only mildly impaired the usefulness of his memoir.

The basic outline of what Sorensen has to say does not differ markedly from the contemporary outpouring of first-rate reporting; there was never much of a barrier between the Kennedy administration and the press. Much valuable detail is added, some of it from materials to which Sorensen had access, for example, Richard Neustadt's historical account of the Skybolt-Nassau-MLF negotiations with the British. Some of the most interesting and exciting passages are first-hand. Thus the President, comparing the Soviet threat to West Berlin with the troubles with Fidel Castro, said to Sorensen, "If we solve the Berlin problem without war, Cuba will look pretty small. And if there is a war, Cuba won't matter much either." As the Soviet government began to build missile sites in Cuba, the two problems coalesced. On the eve of the crisis, early in September 1962, Sorensen lunched with the Soviet ambassador and conveyed to Kennedy notes he took on a personal message from Khrushchev that the ambassador read: "Nothing will be undertaken before the American Congressional elections that could complicate the international situation or aggravate the tension in the relations between our two countries . . . provided there are no actions taken on the other side which would change the situation." In details like these are the merit and interest of Sorensen's *Kennedy*.

Harvard University

FRANK FREIDEL

DICTIONARY OF CANADIAN BIOGRAPHY. Volume I, 1000 TO 1700.
([Toronto:] University of Toronto Press. 1966. Pp. xxiii, 755. \$15.00.)

WHEN Mr. James Nicholson, a Toronto businessman, proprietor of Brock's Bird Seed, and bibliophile, died in 1952, his will provided funds for the financing of a Canadian equivalent to the *Dictionary of National Biography*, an enterprise he much admired. Through his widow's encouragement, the University of Toronto Press undertook the publication, and George W. Brown, professor of history at the University of Toronto, was appointed editor. In 1961 *Les Presses de l'Université Laval* agreed to publish, with the help of the Canada Council, a

French edition as well, and now, with Brown unfortunately not alive to see the first fruits of his labors, we have Volume I of the *DCB/DBC*.

A biographical dictionary succeeds or fails long before a page is printed, for there are manifold editorial decisions to be made, each of which will bind succeeding writers. Virtually all the decisions Brown and his associates made were wise ones. Turning away from the alphabetical arrangement of the *DNB* and the *DAB*, which raised problems of supplementary volumes, and which meant that no historical period was complete until the entire series was published, Brown chose to bring out volumes by periods, alphabetically within the period, with inclusion in a particular volume to be determined by a subject's date of death. Thus, all those figures significant to Canadian history who died on or before 1700 are included within the present book. This arrangement produces problems, of course, for we have no essay on Denonville or Champigny, for example, who died in 1710 and 1720, respectively, but whose whole significance for Canada falls well into the seventeenth century. But had an alphabetical arrangement been used, we should have waited years for the brilliant sketch of Jean Talon by André Vachon in this volume. Clearly Brown's choice was the right one. There will be eighteen to twenty volumes in all.

The first volume is virtually an unqualified success. Handsomely printed and sturdily bound, it contains useful essays on "The Indians of Northeastern North America," "The Northern Approaches to Canada," and "The Atlantic Region," and a superb summary of "New France, 1524-1713," the last written by the associate editor, Marcel Trudel. Each of the biographical sketches includes a list of sources and authorities generally far superior to those in the *DNB*. Some of the essays are models of compression and clarity: that on Paul Le Jeune, originator of the "Jesuit Relations," written by Léon Pouliot, is the finest on its subject I have seen. Inevitably there is some repetition—twice we learn that Eustache Ahatsistari was a great warrior—but the cross-referencing is extremely well done. The editors and the 115 contributors have begun an important task well. With the almost simultaneous publication of the first volume of the *Australian Dictionary of Biography, 1788-1850*, students of the Commonwealth at last have the beginnings of the kind of tools their colleagues in British and American history long have used.

Yale University

ROBIN W. WINKS

MANITOBA: THE BIRTH OF A PROVINCE. By *W. L. Morton*. [Manitoba Record Society Publications, Volume I.] ([Winnipeg: the Society.] 1965. Pp. xxx, 265.)

THIS is an interesting collection of documents on the negotiations between the people at Red River and the Canadian government, which led to the settlement's admission to confederation as the province of Manitoba. Some of the documents have long been in print, but are not readily available; others, including the diary of Sir Stafford Northcote, governor of the Hudson's Bay Company, are published for the first time.

The settlers had not been consulted about the transfer of Rupert's Land from

the Hudson's Bay Company to the Dominion of Canada, and they were afraid that Canada would not recognize their rights. Under Riel's leadership, the métis from the French parishes refused to allow the Canadian governor-designate to enter Rupert's Land. The Canadian government then sent missions of conciliation to Red River, and one under Donald A. Smith, the chief Hudson's Bay Company officer at Montreal, was successful. The settlers elected representatives who met in convention and discussed the terms of transfer. Later they sent three delegates with their terms to Ottawa. The full text of Smith's confidential report to the Canadian government is included in this volume, along with extracts from the proceedings in the Red River convention.

While there are no official records of the conferences with Sir John A. Macdonald and Sir George Cartier, one of the Red River delegates, the Reverend N. J. Ritchot, kept a journal. Professor Morton has provided a translation of this important journal. The views of the main negotiators are recorded in the diary of Northcote and in the letters of a special agent from the State Department at Washington, J. W. Taylor. Northcote came from London to watch the proceedings, and, since he moved freely in government circles, his comments on Canadian politics are valuable. Taylor also had access to good sources of information, and he sent accurate reports to the Secretary of State, Hamilton Fish. Extracts from the House of Commons' debates on the Manitoba bill and documents relating to the completion of the transfer round out the documentation.

Morton's introduction and notes are useful, but they are not as impressive in style and comprehensiveness as those he provided for *Alexander Begg's Red River Journal*, an outstanding volume in the Champlain Society series.

University of Saskatchewan, Saskatoon

JEAN E. MURRAY

THE IMPERIAL POLICY OF SIR ROBERT BORDEN. By *Harold A. Wilson*.

[University of Florida Monographs, Social Sciences, Number 29.] (Gainesville: University of Florida Press. 1966. Pp. 76. \$2.00.)

THIS useful monograph, well prepared and well presented, nice in statement and judicious in temper, probably says all that need be said, in main outline, of the imperial policy developed by Borden while Prime Minister of Canada. There is relatively little in it that is new, and that little confirms or supplements what was already known.

Mr. Wilson's theme, broadly stated, is that at the beginning of Borden's prime ministership, the chief issue of Canadian external policy was that of its relation with the British Empire. Despite the growth of responsible government and colonial nationalism, the Empire was still in all matters of foreign policy a unitary state, both legally and practically. Yet the statesmen of the self-governing colonies, in the event of war, would be responsible to their Parliaments for men and money committed to further foreign policies in the making of which they had taken no part. Borden began his period of office by endeavoring to have Canadian participation in imperial defense made tolerable by providing Canada with "an adequate voice" in the conduct of imperial foreign policy. When he failed to accomplish this in the course of the First World War, he became convinced, although the

leader of the Conservative party of Canada—the “imperialist” party—that only “autonomy” would resolve the dilemma of responsibility without representation. He became in fact the chief architect of the semi-independence of the Dominion after 1918.

Wilson very effectively crowns his lucid discussion of this development with the noteworthy and, to me, acceptable judgment: “Borden’s contributions to the constitutional development of the Empire were far greater than any combination of his contemporaries.” Only a study of his chief aid, Loring Christie, can add much to the Canadian side of this particular theme.

Trent University

W. L. MORTON

DIVIDING THE WATERS: A CENTURY OF CONTROVERSY BETWEEN THE UNITED STATES AND MEXICO. By *Norris Hundley, Jr.* (Berkeley and Los Angeles: University of California Press. 1966. Pp. xii, 266. \$6.95.)

AN important but little-noticed aspect of American-Mexican boundary relations is the complicated problem of sharing the flow of water in international rivers. Many confuse this question with the better-known Chamizal controversy over riverbank lands in and around El Paso. Professor Hundley’s brief, tidy monograph clears up this confusion, defining the problem of irrigation waters and clarifying the two nations’ efforts to work out a fair division.

The controversy has actually involved four distinct regions: the upper and lower Rio Grande Valleys, the Colorado River Basin, and the much smaller Tijuana River system, supplying San Diego. Each region presents its own complexities. In the cases of the Colorado River and the upper Rio Grande, for example, the United States furnishes most of the water, while the largest tributaries of the lower Rio Grande lie in Mexico. By far the most troublesome of the four problems has involved the Colorado River, for during the early 1940’s the rapidly growing population of southern California placed almost irresistible pressure on the US government to deny to Mexico water, which a treaty of 1904 seemed to promise. In 1944 the two countries attempted a “package” solution of this controversy and another involving the lower Rio Grande in a new treaty that eventually won Senate approval over the loudly protesting bodies of the two California senators. Even this solution already seems inadequate, for Hundley concludes with a sketch of the bleak future confronting water planners in the Southwest.

While much of the book involves complex hydrographic and diplomatic problems requiring close attention from the reader, it makes a significant contribution in several fields, including hemispheric relations, conservation, and federal-state relations. In the last-named field it sets forth a classic illustration of the no man’s land between federal and state influence over foreign relations comparable to the treatment of Japanese residents in early twentieth-century California. Most of Hundley’s material comes from American sources, and although the American side of the problem was indeed the more complex, a little wider use of Mexican data would have been advisable. The reader would probably also benefit from fuller dis-

cussion of the problems of irrigation-based agriculture and the water needs of Los Angeles and San Diego, as well as a better description of the All-American Canal supplying the Imperial Valley. The many well-drawn maps of river basins and canals deserve special praise.

Indiana University

DAVID M. PLETCHER

THE EARLY SPANISH MAIN. By *Carl Ortwin Sauer*. (Berkeley and Los Angeles: University of California Press. 1966. Pp. xii, 306. \$7.95.)

HISTORY written by a geographer, especially one of Professor Sauer's learning, is an enriching experience to read. This book is more than an account of Spanish activities in the Caribbean from 1492 to 1519. It also covers the fauna and flora, the ecological changes in the area of the period, and most especially the disastrous effects upon the Indians.

Sauer states in his foreword that "a scant thirty years of discovery and domination gave not only geographical but large economic and political direction to Spanish empire. I have tried here to outline and interpret the historical geography of this brief period and narrow region of the New World." One can say with assurance that his attempt is successful.

The first voyage of Columbus opened a new empire to the Spaniards as well as a new world to Europe. The four voyages of Columbus, the occupation of Española, the expansion from that island to others of the West Indies, the exploration of the northern coast of South America and Panama, Balboa's crossing of the isthmus, and the colonization of Castilla del Oro and the Pacific coast of Panama are covered with a lucid style and a wealth of detail.

The Spanish obsession for gold and their cruel ruthlessness in the extermination of the Indians of the islands (mostly in their efforts to obtain gold) are horrifying; the native population was destroyed in scarcely thirty years. Sauer does not gloss over the matter. Only the lone voice of Las Casas was raised against the treatment of the Indians for humanity's sake, and only that of Balboa and a few others occasionally for purely practical reasons.

The Spanish record for this period in the Caribbean is appalling. But considering the remoteness from home government, and, in general, the type of men involved, one is only surprised that the period did not last longer. The reasons that it did not are well summarized by Sauer. "By 1519 the Spanish Main was a sorry shell. The natives, whom Columbus belatedly knew to be the wealth of the land, were destroyed. The gold placers of the islands were worked out. The gold treasures which the Indians of Castilla del Oro had acquired had been looted."

The Spaniards learned, however, from their West Indian and Spanish Main apprenticeship. Men of higher caliber, more able, more humane, administered the expanding Empire. The efficient administrations enjoyed for many years in Mexico and Peru are in sharp contrast to the early governing of the Caribbean lands.

Peabody Museum of Salem

ERNEST S. DODGE

CUBA, HAITI, & THE DOMINICAN REPUBLIC. By *John Edwin Fagg*. [The Modern Nations in Historical Perspective. Spectrum Book.] (Englewood Cliffs, N. J.: Prentice-Hall. 1965. Pp. viii, 181. Cloth \$4.95, paper \$1.95.)

Mr. Fagg has written an outstandingly successful history of these three Caribbean countries. He has covered in 180 pages over four centuries in the lives of peoples who have substantially less of a shared or even similar past than geographical propinquity would seem to dictate. This is no mean feat. Almost two-thirds of the volume is on Cuba, a proportion fully justified by the paucity of sources for the other two, if not by their relative importance. Greater stress is placed upon national than colonial Cuba, and, throughout, Cuban opinion on US policies is quoted and assessed. The crucial period 1950-1959 is here presented in the most balanced synthesis I have seen. In contrast to Theodore Draper's often polemical approach, Fagg weighs the many advances of the era against the prolonged problems and the intellectual unrest that contributed so heavily to Castroism. The briefer sections on Haiti and the Dominican Republic contribute to our understanding of the "why" of the lamentable conditions there.

I must disagree with Fagg on two points of interpretation regarding Cuba. The statement that "Second-level policy-makers in the American department of state virtually supported Castro . . ." is not correct in my view. The prevailing opinion was certainly not pro-Castro and did not favor Batista either. And, contrary to the author's assertion, Castro's guerrillas did indeed "best" the Batista forces in battle by using traditional guerrilla tactics against a demoralized army, although the collapse of the regime was also political; that is, the Cuban people no longer supported it. Admittedly, these are basic questions of historical interpretation on which it is too early to give a definitive judgment. The full story of US policy is not yet public, although we have been tantalized by various revelations such as the evidence in a recent congressional hearing that the CIA had its own foreign policy in Cuba. Only now are rich sources for the history of Castro's guerrilla campaign in 1957-1958 becoming available.

The annotated bibliography is comprehensive and useful. By handling his raw material judiciously, Fagg has provided the reader with a most interesting and excellent new summary of the history of these three countries.

Indiana University

DAVID D. BURKS

SEGUNDO CENTENARIO DEL NACIMIENTO DE DON ANTONIO NARIÑO, 1765-1965. [Biblioteca de historia nacional, Volume CVI.] (Bogotá: Editorial Kelly. n.d. Pp. 213.)

COLECCIÓN DE DOCUMENTOS PARA LA HISTORIA DE COLOMBIA. Second Series. Compiled by *Sergio Elías Ortiz*. [Biblioteca de historia nacional, Volume CV.] (Bogotá: Editorial Kelly. 1965. Pp. 323.)

THE first of these books records the public ceremonies with which the Colombian Academy of History commemorated, in the spring of 1965, the second centenary of the birth of Antonio Nariño, precursor of Colombian independence. About a quarter of the volume is devoted to formal resolutions of support by various public and private groups; the balance is given over to the texts of patriotic discourses

presented on various occasions during the celebration. As the occasion required, these addresses are concerned more with evoking the patriotism of the audience and lauding one of the founders of the republic than they are to giving a critical and comprehensive view of Nariño's life and works. The various addresses cover the facts of Nariño's life, but have comparatively little to offer in the way of critical historical analysis. Perhaps the most informative and thoughtful addresses are those by Carlos Restrepo Canal on "Nariño Periodista y Hombre de Estado" and Albert Lleras Camargo, "Evocación del Prócer." These contributions do offer some interpretive insights.

The second volume offers a collection of documents bearing upon revolutionary Colombia. The book begins by giving us a fascinating, half-shaded glimpse of the conspiratorial world of Nariño's contemporaries and associates, Pedro Fermín de Vargas and Luis Rieux. Next we get a look at the other side of the hill in the memorials of a loyalist canon, Andrés Rosillo. The editor then presents a group of letters concerning the military operations and the situation of the Spanish preceding and following the Battle of Boyacá. The final groups of documents embrace the diplomatic correspondence of New Granada's representative in London, an exchange of letters between the Congress of Bogotá and the dissident government of Santa Marta, and also one between the Cartagena government and the Spanish Cortes.

These documents, all drawn from the *Archivo General de las Indias*, are well chosen and presented; they penetrate the complexities of the independence movements and illuminate, albeit fitfully, the careers of some important, though obscure personages. On the adverse side, I cannot help but wish for a somewhat more complete editorial apparatus. The identification of persons as they appear and an index extending beyond proper names would be welcome to most readers.

To summarize, the first volume will probably interest only those who are interested in the shades of modern interpretation of Nariño. The second is a valuable contribution of published documents that will interest, in some degree, almost everyone concerned with Colombian history or the independence movements in Hispanic America.

Brigham Young University

GEORGE M. ADDY

SLAVE SOCIETY IN THE BRITISH LEEWARD ISLANDS AT THE END OF THE EIGHTEENTH CENTURY. By *Elsa V. Goveia*. [Caribbean Series, Number 8.] (New Haven, Conn.: Yale University Press. 1965. Pp. ix, 370. \$8.50.)

THIS notable monograph is an expansion of an English dissertation. It seeks to portray the society emerging in a British Caribbean island cluster where, by 1800, slavery was an ancient and fully developed institution about to undergo serious challenge, an objective the author has admirably attained.

A minute examination of the community's political structure, the local sugar industry, and its servile regime is followed by a detailed consideration of its three basic population elements: small numbers of plantation-owning masters returning permanently to Britain at the slightest opportunity; bondsmen largely island-born;

and a discordant body of manumitted freemen, heavily mulatto. Outstanding is a penetrating analysis of the diverse forces bringing into being a distinctive civilization and a new way of life. Essentially a well-balanced, socioeconomic treatise based upon contemporary materials and written in a highly pleasing style, Professor Goveia's volume is one of great merit certain to gain honored rank among key works on modern colonization.

The political structure of the islands was indeed archaic. While under a governor in chief, Antigua, St. Kitts, Nevis, Montserrat, and the Virgin Islands all had separate legislatures in weakening white hands and given to exaggerated parochialism. A joint body for common affairs seldom met and was completely ineffectual. Each island, too, had its own administrators, courts, and militia, compounding the general inefficiency. By 1800 the small cotton and sugar growers had given way to large cane producers, and perilous monoculture prevailed. Competition from other areas and rising costs plagued the proprietors and steeled them against humanitarian movements arising in Britain that likewise menaced the slaves' status since estate economies and income maintenance bore heavily upon them. A population of 8,000 whites, 81,000 slaves, and 2,600 free blacks lent itself to tensions assuming ominous aspects in a time of financial strain.

Briefly, the wholesale departure of wealthy planters, merchants, and professional men widened economic opportunity for resident poor Europeans, vastly narrowed former gaps between different levels of Leeward whites, and united them solidly against both freemen and slaves. The freemen, grown in numbers, had become restless and demanding, following a movement to curb their liberties. While of diverse African origin, close association in the same New World environment broke down initial differences between slaves and created a new prevailing type, the Creole black, which, being island-born, was less docile than the early importees had been. Strikingly enough, through constant effort to placate the whites, the missionaries of the era were actually bolstering slavery rather than undermining it.

All in all, this is a highly informative and challenging presentation.

Ohio State University

LOWELL RAGATZ

VOINA ZA NEZAVISIMOST' MEKSIKI (1810-1824) [The Mexican War of Independence (1810-1824)]. By *M. S. Al'perovich*. (Moscow: Izdatel'stvo "Nauka." 1964. Pp. 476.)

ROSSIJA I VOINA ZA NEZAVISIMOST' V ISPANSKOI AMERIKE [Russia and the War of Independence in Spanish America]. By *L. Iu. Slezkin*. (Moscow: Izdatel'stvo "Nauka." 1964. Pp. 381.)

THESE books are similar in their faithful Marxist and current party line orientation, but a closer analysis reveals a new dimension in Soviet scholarship. Al'perovich presents for the first time a well-developed interpretation of the leading figures and problems of the Mexican War of Independence, while Slezkin offers an original, carefully wrought study of Russian diplomacy during the revolutionary period in Latin America and of the impact of these events on the Russian intelligentsia.

Both works are characterized by excessive documentation and a disparagement of non-Marxist writers; Al'perovich's concluding historiographical essay, however, is generally free of dogma as he summarizes the many schools of thought that have assessed the Mexican independence struggle since its inception. Slezkin's inclusion of original and often unpublished tsarist Russian archival materials renders his study a major contribution not only to Russian but also to European and Latin American diplomatic history. His book merits translation and consultation by scholars in the Western Hemisphere.

The volume by Al'perovich contains a preface, eight chapters, a historiographical essay, a bibliography, an index, and a brief summary in Spanish. He argues effectively that the severance of Spanish colonial dominion and the formal abolition of feudalism in Mexico did not lead to fundamental changes in the country's socioeconomic structure. The *latifundistas* and the Catholic Church preserved their privileged economic status, and the masses of Indian peasants, their suffrage effectively restricted, did not benefit from the programs proclaimed by Hidalgo and Morelos, or from the emergent liberal tradition. Furthermore, conditions favoring the growth of capitalism in the domestic and international affairs of Mexico were enhanced by an essentially bourgeois revolution not intended to resolve fundamental social problems.

Slezkin's book comprises a brief introduction, an essay on the availability and use of primary and secondary sources, eight chapters assaying the development of Russian concern with the wars of liberation in Spanish America, a bibliography, and an index of names and places. Slezkin examines first the period 1810-1812, when Alexander I and his ministers attempted to take advantage of Spain's predicament in the New World and in Napoleonic Europe and strove to establish diplomatic and economic ties with Latin America. The second period, 1812-1816, is characterized by the reaction that swept Europe and was epitomized by the Russian-inspired Holy Alliance. The final period, 1817-1825, is marked by an ambivalence in official Russian policy as Alexander sought to prevail as "gendarme of Europe" while his ministers increasingly recognized the impossibility of suppressing Latin American insurgency by force. This period also witnessed the expression of sympathetic support for Latin America's wars of independence by Russia's intelligentsia, particularly the Decembrists.

The interpretations of Al'perovich and Slezkin, though controversial and vulnerable, nevertheless represent an interesting contribution to hemispheric historiography.

University of Arizona

J. GREGORY OSWALD

EL PENSAMIENTO CONSTITUCIONAL DE LATINOAMÉRICA, 1810-1830. In five volumes. [Biblioteca de la Academia Nacional de la Historia, Numbers 47-51. Congreso de Academias e Institutos Históricos sobre el Pensamiento Constitucional de Latinoamérica, 1810-1830. Actas y ponencias, Volumes I-V.] (Caracas: the Academia. 1962. Pp. 415; 441; 380; 402; 430.)

In 1961 the Venezuelan Academy of History organized a conference on the history of political and constitutional ideas in Latin America, 1810-1830, as part of

the celebration of the sesquicentennial of the declaration of independence of the republic. The present volumes (published in 1962, but which, for some unknown reason, did not reach this country until 1965) contain the speeches delivered at the conference, the minutes of the various formal and discussion sessions, and the texts of the papers presented. The five volumes form part of a large collection of sources and monographs published by the academy in honor of the sesquicentennial—a noteworthy contribution to the study of the movement for independence in Spanish America.

The conference brought together about forty scholars from thirteen Latin American countries, the United States, Sweden, France, Germany, and Spain. Papers were also submitted by others unable to attend the gathering. A majority of the contributions are respectable essays on diverse aspects of the political thought of the revolutionary era: the influence of antecedent ideas—French, British, North American, and Spanish (with much attention to the influence of the seventeenth-century writer, Francisco Suárez); the relation of specific provisions of various Latin American constitutions of the era of independence to previous constitutional documents—French, American, and Spanish (of considerable interest is the use of the various Napoleonic constitutional documents); the role of federalism in the constitutional doctrines of the time; relations of church and state; and, finally, inter-American cooperation and confederation.

Although many of these essays include useful information not always conveniently available before, they cannot be said to make new and significant contributions to the historiography of the independence period. An exception can perhaps be made to this statement in connection with the treatment of the idea of “the rights of man” and the particular French formulations of the principle. This topic is dealt with in many papers, and conjointly they furnish an extensive study of the subject. It would be futile here to single out particular essays for comment. One general observation can be made, however: the traditional pious rhetoric associated with such celebrations appears to be giving way, gradually, to a more serious and critical approach to the history of the independence movement in Latin America.

Vassar College

CHARLES GRIFFIN

LA INTERVENCIÓN FRANCESA Y EL IMPERIO DE MAXIMILIANO: CIEN AÑOS DESPUÉS, 1862–1962. ESTUDIADO CIEN AÑOS DESPUÉS POR HISTORIADORES MEXICANOS Y FRANCESES. Edition prepared by *Arturo Arnáiz y Freg* and *Claude Bataillon*. (México, D. F.: Asociación Mexicana de Historiadores, Instituto Francés de América Latina. 1965. Pp. 217. \$3.50.)

As director of the French Institute of Latin America in Mexico, François Chevalier initiated the program of the Round Table of Mexican Social History over which Mexican historian Arturo Arnáiz y Freg presides. Over the years the sessions have been well attended; the papers presented, often interesting and at times provocative; and the discussions, frequently spirited and illuminating. Now, for the first time, a series of presentations, those constituting the 1962 series during which

Mexican and French scholars viewed aspects of the French intervention and Maximilian's empire a century earlier, appear in published form.

The twenty-five selections vary widely; they include bibliographic introductions by Luis Chávez Orozco, Ernesto de la Torre Villar, and Juan Ortega y Medina, monographic studies, and analytical essays of interpretation. Brief French summaries follow the articles by Mexican scholars, and Spanish ones accompany the two selections contributed by French authors. Unfortunately, only in one instance is an effort made to provide the reader with a sample of the resulting discussion.

In the introductory essay Chevalier provides a fine example of the analytical contribution with his perceptive examination of the sociology and political geography of liberalism and conservatism in Mexico. The studies of Luis González y González and Antonio Martínez Báez categorize Maximilian's policy as liberal. Moisés González Navarro contributes an incisive characterization of the reform and empire periods while Wigberto Jiménez Moreno views May 5 as signifying the recuperation of national pride.

Three monographic studies are represented by Xavier Tavera's splendid examination of the economic consequences of the intervention, Daniel Cosío Villegas' knowledgeable presentation of Franco-Mexican relations, 1867-1880, and Frédéric Mauro's study of the role of the northeastern economy in the resistance to the Empire. Finally, there are four contributions related to cultural aspects or consequences of the French intervention by Vicente T. Mendoza, Francisco Monterde, Mauricio Gómez Mayorga, and Manuel Maldonado Koerdell.

There is much of interest in this volume. It is a worthy publication for this reason and as a tribute to Chevalier who, in his role as director of the institute, provided through the Round Table a propitious environment for the presentation and discussion of historical studies.

State University of New York, Stony Brook

STANLEY ROBERT ROSS

TRUJILLO: THE LIFE AND TIMES OF A CARIBBEAN DICTATOR. By Robert D. Crassweller. (New York: Macmillan Company. 1966. Pp. xii, 468. \$8.95.)

WRITTEN with exceptional literary skill keenly attuned to the dramatic, this is an intriguing and vivid description of one of Latin America's most repugnant and fascinating dictators. Scarcely ever, through the five hundred pages of text, does the interest of the reader lag, for the author embellishes his chronicle of the rise, consolidation, and fall of the Trujillo tyranny with copious illustrations of unscrupulous maneuvering, betrayals, and murders that challenge credulity.

Undoubtedly most of this is true, but it is impossible for the reader to measure the accuracy of the blanket indictment of Trujillo, for this is not a documented history. Mr. Crassweller refrains by so much as a single footnote to offer verifiable evidence in support of his numerous conclusions asserted as historical fact. Thus we are told without any documentary proof that Trujillo "gave the order to launch the famous massacres" of October 2, 1937, on the Haitian-Dominican border. We are also told repeatedly, and without support of authority, that

Trujillo ordered this or that murder or other foul deed. The author seldom qualifies his categorical affirmations; he asks us to accept his conclusions on faith.

The kind of evidence particularly treasured by Crassweller is the recollections of individuals whose identities remain anonymous. These personal recollections, after the lapse of many years, are sources of quoted conversations freely used in the text, and of interesting and intimate facts of character and behavior that must have been privy only to the individuals concerned or to their mistresses.

Although the biography of Rafael Leonidas Trujillo must be concerned with intrigue, violence, graft, and immorality, it seems that Crassweller could have spared a few of the pages concerning sexual excesses to credit the devil his due. How did Trujillo accomplish the early payment of the republic's foreign debt? Should not more than two pages be devoted to the extensive material improvements that Trujillo wrought? Is not the matter of how Trujillo raised the standard of living to the highest point before or since his regime worthy of discussion? Are not these matters as worthy of discussion as the unsavory behavior of the Trujillo family?

University of Texas

J. LLOYD MECHAM

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Donald E. Worcester, Texas Christian University

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* * * * Association Notes * * * *

AMERICAN HISTORICAL ASSOCIATION

The *List of Doctoral Dissertations in Progress or Completed at Colleges and Universities in the United States*, a triennial publication of the Association, is scheduled to appear again in 1967. The 1964 *List* is available from the Association for \$1.50. A current file of titles of dissertations in progress is maintained at the Association's office; forms for registering topics are available to individuals or departments upon request. Those registering titles of dissertations in progress should check previous *Lists* to prevent duplication. If a topic is changed, the Association should be informed of both old and new titles.

The Association's *Annual Report* contains a list of completed dissertations, for which a degree has been awarded, each year.

The Service Center for Teachers of History now has nearly seventy pamphlets in its series. Most recently published are Wilcomb E. Washburn, *The Age of Discovery*; R. K. Webb, *English History, 1815-1914*; Edwin S. Gaustad, *American Religious History*; and Jack P. Greene, *Revolutionary America*. Twenty-five titles are now in second or third editions, with other revisions in progress; a dozen new titles have been commissioned.

Pamphlets are priced at fifty cents each, with a discount of 20 per cent offered on orders of fifty or more. Payment must accompany all orders—with one exception: educational institutions submitting orders of ten dollars or more need not enclose payment. Checks should be made payable to the Service Center for Teachers of History; orders should be addressed to the Service Center at 400 A Street, S.E., Washington, D. C. 20003.

RECENT DEATHS

Enoch L. Mitchell, professor at Memphis State University, died December 25, 1965, at the age of sixty-two.

William M. Jordan of Port Washington, New York, died January 10, 1966.

On February 25, Franz Schnabel, professor of medieval and modern history at the University of Munich and an honorary member of the American Historical Association, died at the age of seventy-nine. He was born in Mannheim, the city to which his loyalty belonged and which in 1954 made him an honorary citizen. He began his academic career in 1920 at the Institute of Technology in Karlsruhe, where he became a professor in 1922. In 1936 the Nazis removed him from his office for political reasons. In 1947 he was called to the University of Munich

where he exercised great academic influence through his large lecture courses and seminars. In 1964 he became an emeritus professor.

Schnabel's heart belonged to pre-Bismarckian Germany, and, though he lectured and published on all ages of modern German history, his chief research and writing belonged to the period 1750-1850. His early studies were concerned particularly with the formation of the new southern German states in the early nineteenth century, but also showed interest in the history of education, which characterized his subsequent work. His chief work was his *German History in the Nineteenth Century* (1929-37). It broke completely not only with the *kleindeutsch* view of German history but also with the exclusive concentration on political history. Schnabel wanted to show how man was changed not only by political events but also by the impact of ideas and material circumstances. The third and fourth volumes are really special monographs on the impact of the natural and humanistic sciences as well as of the Roman Catholic and Protestant churches. Together they lay a firm foundation for the treatment of nineteenth-century German history.

Daniel C. Knowlton, professor emeritus at New York University, died March 5, at the age of eighty-nine.

Arthur I. Bernstein of Brooklyn, New York, died April 5, at the age of forty-three.

F. D. Nichol, editor of the *Review and Herald*, Washington, D. C., died June 3.

Chester Penn Higby, professor emeritus at the University of Wisconsin, died June 26, at the age of eighty. Born in Ottawa, Illinois, on October 27, 1885, he received his bachelor's and master's degrees from Bucknell University, where he also later received an honorary degree; in 1919 he obtained his Ph.D. degree from Columbia University. He taught at a number of institutions before joining the faculty at the University of Wisconsin in 1927, at which time he was working to found a scholarly review devoted to the publication of articles and book reviews in the field of modern European history and to secure constituent membership for scholars in that field within the broader American Historical Association. His efforts were rewarded with the appearance in 1929 of the *Journal of Modern History* and the establishment in 1930 of the Modern European History Section of the American Historical Association. He was its first president. Higby was the author of *The Religious Policy of the Bavarian Government during the Napoleonic Period* and *History of Europe: 1492-1815*.

Frederick W. Loetscher, professor emeritus at Princeton Theological Seminary, died July 31.

Benjamin H. Pershing of Carrollton, Georgia, died August 4

Richard L. Beyer of Gannon College, Erie, Pennsylvania, died August 7.

The death of Robert Livingston Schuyler, professor emeritus at Columbia University, on August 15, has left our historical company poorer. He was editor of the *American Historical Review* from 1936 to 1941 and President of the American Historical Association in 1951. To those who knew him he was a very great gentleman who taught his students as much by his personality as by didactic instruction. His unerring taste played conscience to them, for when tempted to indulge in some dubious metaphor, dubious alike in relevance and propriety, many a student excised his flight on asking himself: "What would RLS have said to that?" Kind but never soft, he had no patience with pretense and humbug. Never claiming overmuch for himself ("I am," he once remarked, "a writer of articles, not of books."), he could appraise his contemporaries and earlier historians as well. No one can read his essays on Green and Macaulay and, above all, his hero, Maitland, and fail to sense his perception of quality. No one can read the essays in *Parliament and the British Empire* without appreciating that a sure guide is leading him through intricate mazes. At his best with a limited subject, Schuyler had the knack of distilling from accessible materials applications beyond the immediate issue. At the same time he was constantly alive to the quicksands of anachronism, where his sensitivity embraced both things and words and prompted him to condemn the jargon that labels everything and comprehends nothing. He did not waste words; nor did he debase them. Among all the qualities that drew him to Maitland—luminous, concrete, sensitive, ever so learned—none appealed to him more than that great historian's concern with the proper use of words and his awareness of the grave dangers, far beyond mere semantics, in disregarding changes in meaning. These qualities Schuyler passed on to his students, and, if such qualities are less striking than novel interpretations or monumental generalizations, they are no less to be cherished. In the surest way he taught his students—and his readers too had they the wit to see—not merely the rudiments but the very being of historical study.

John K. D. Chivers of Lawrenceville, New Jersey, died in August.

Delio Cantimori, professor of modern history at the University of Florence and an honorary member of the American Historical Association, died September 13, at the age of sixty-two.

Cantimori came to history from the study of literature and of theology, and his work always remained focused on issues of intellectual history. But this interest was combined with great sensitivity to social issues and a lively concern for the social relevance of ideas. Thus, Cantimori's masterwork, the *Eretici italiani del Cinquecento* (1939), places the individual figures of the Italian reformers into the broad framework of the intellectual and social history of the entire sixteenth century. In his *Prospettive di storia ereticale italiana del Cinquecento* (1960) Cantimori set forth a comprehensive and stimulating program for the pursuit of further researches in this field. Cantimori's interests extended far beyond the sixteenth century, however. In his *Utopisti e riformatori italiani 1794-1847* (1943)

he investigated the role that ideas of social radicalism played in the history of the *Risorgimento*. A volume of essays, *Studi di storia* (1959), shows the remarkable extent of Cantimori's historical concerns.

Cantimori's scholarly work was inspired by a deep respect for the uniqueness of each human personality, and this expressed itself in the particular attention with which he studied the ideas of heretics, outsiders, and minorities. Because of the breadth of his views he attracted a great number of students to whom he gave unsparingly of his time; American historians working in Italian history will gratefully remember the interest and encouragement they received from him.

John H. Kent, Roberts Professor of Classical Languages and Literature at the University of Vermont, died September 27.

Waldo Gifford Leland, friend and leader of scholars, died October 19. Born in Newton, Massachusetts, July 17, 1879, he was of the second generation of professional historians. His passing breaks one of the last links of the present generation with the giants of the first. A student of J. Franklin Jameson and long one of his chief assistants at the Carnegie Institution of Washington, he also knew Henry Adams and in Europe was a friend of Henri Pirenne. A New England gentleman who possessed the best qualities, the dignity and learning, of the late nineteenth century, he came of age just as the twentieth century was beginning and played a leading role in the development of scholarship in the humanities from 1903 to the present.

The offices he held, the work he did, the high standards he maintained, the wise counsel he offered to hundreds of aspiring scholars, all brought him fame, affection, respect, and admiration. Educated at Brown and Harvard he early attracted the attention of leaders of the profession, particularly Jameson's. In 1907 he went to Paris to head a historical mission for the Carnegie Institution, to locate and to prepare guides to materials for American history. From 1907 to 1914 and again from 1922 to 1927 the Lelands often lived in France. He was also Secretary of the American Historical Association from 1909 to 1920. In 1927 he became director of the American Council of Learned Societies, which he had helped to organize, and he remained in that position until 1946. Through these most active years he not only aided scholars in Europe and America; he also received many honors. Called upon by the United States government to be consultant on many missions during the two world wars and the years in between, he always represented the interests of scholarship. Internationally his devotion to these interests brought him in 1938 to the presidency of the International Committee of Historical Sciences. He was always especially proud of his membership in the American Philosophical Society, the American Historical Association, the Society of American Archivists (president, 1939-1941), Phi Beta Kappa, and the Cosmos Club of Washington, D. C. (president, 1947). He never lost his love for France, which made him chevalier of the Legion of Honor, or forgot his obligations to Brown University (Board of Fellows over many years).

Among his many publications were guides to the archives of the United States, to the social and economic history of World War I, and, above all, his *Guide to*

Materials for American History in the Archives and Libraries of Paris (Volume I, 1932; Volume II, 1943; and work in progress).

While Waldo Leland had predecessors of his same mold in the first generation of the profession, it is unlikely that he can have successors of this mold in later generations. But the twentieth-century world of scholarship is richer because he was part of it. And the many scholars he assisted will perpetuate his memory, as does the splendid portrait in the National Archives building.

Dorothy G. Brunk, Gordon Goodman, George H. Nelson, Mrs. Charles C. Ward, and Howard Eugene Wilson, former members of the Association, died recently.

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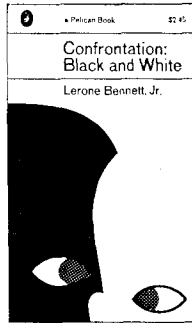
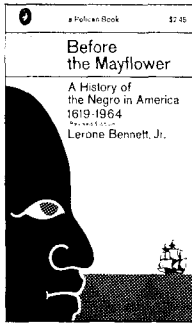
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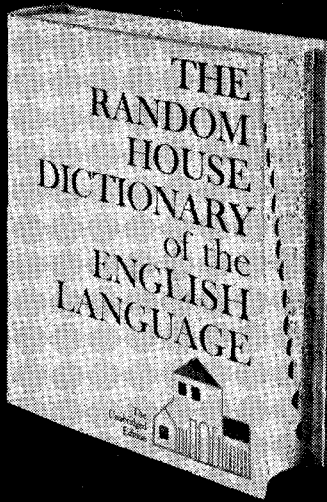
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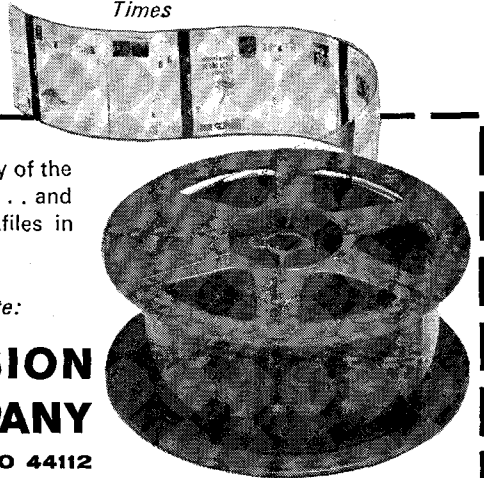
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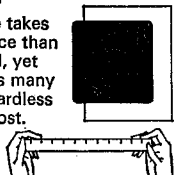


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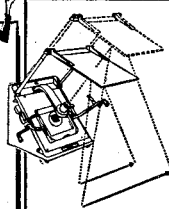
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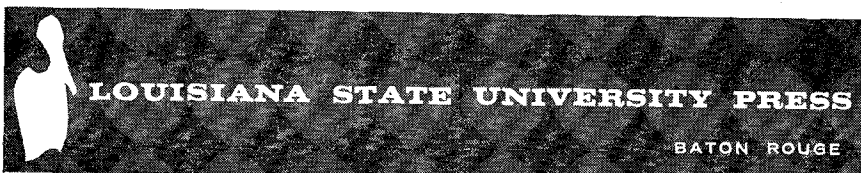
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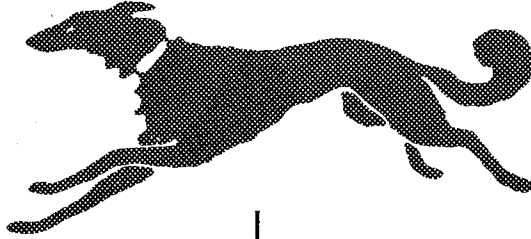
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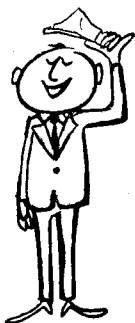
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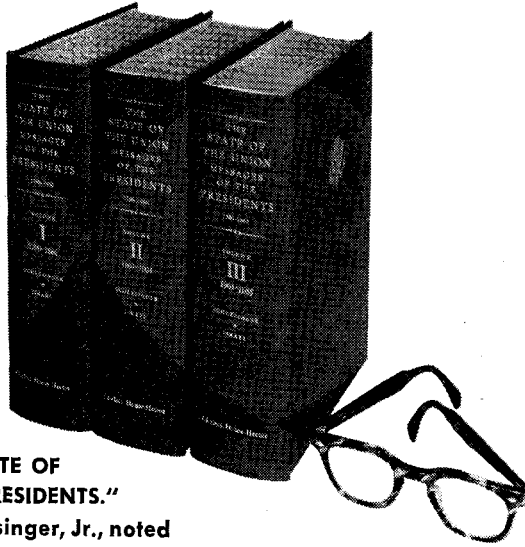
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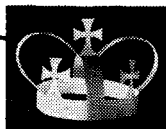
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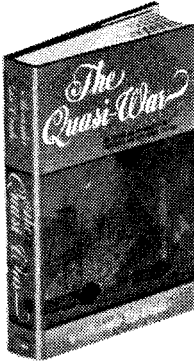
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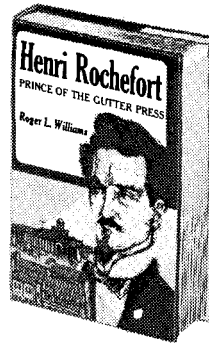
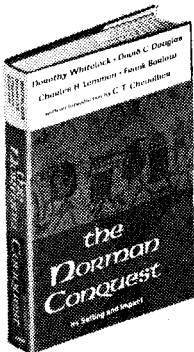
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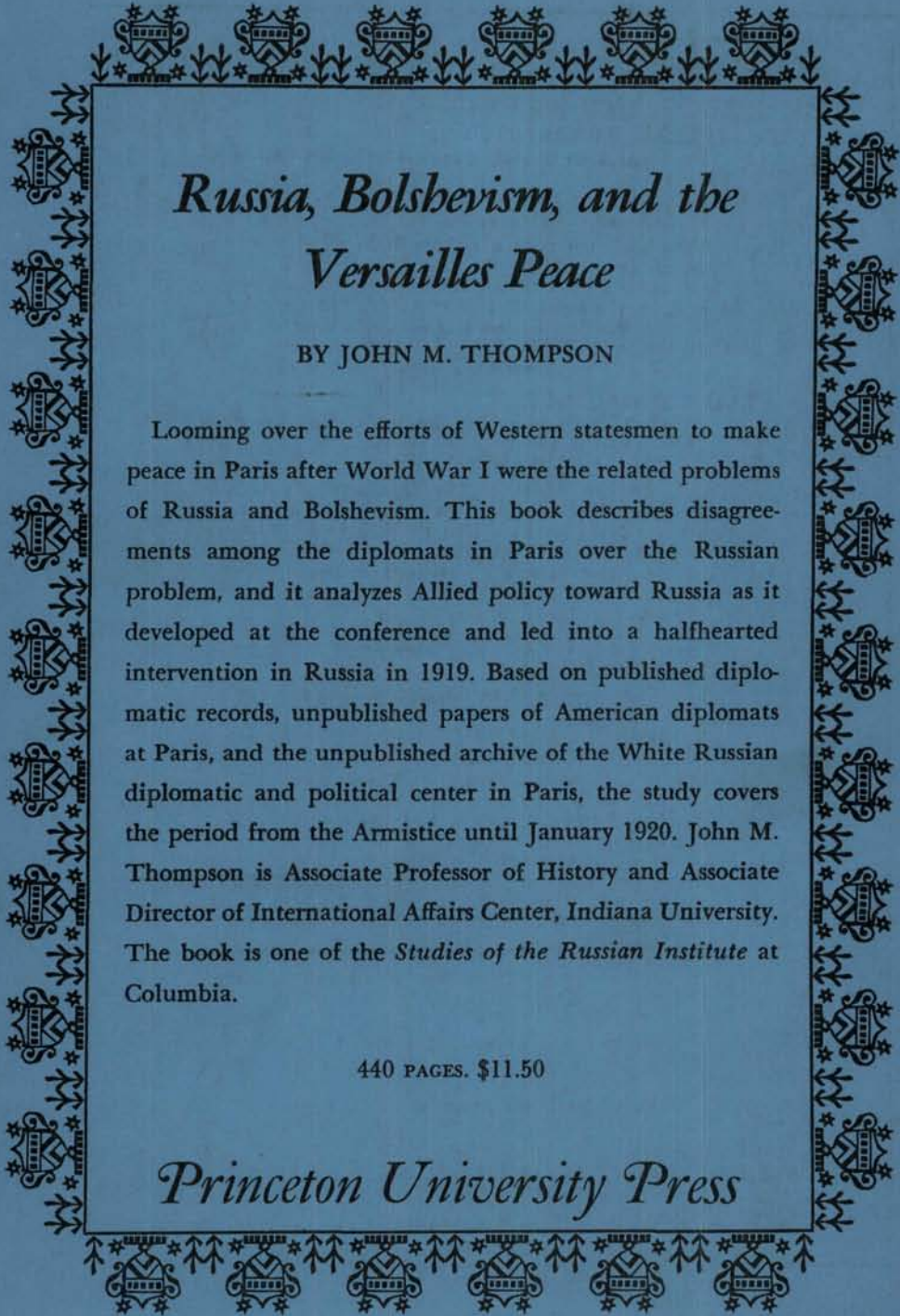
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